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Standardization of Time—Zerubavel

Community Change and Patterns of Delinquency—Bursik and Webb

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Arithmetic of Social Relations—Rytina and Morgan

Using Association Models—Clogg

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Review Symposium on Janowitz—Rieder and Moore

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IN THIS ISSUE

EVIATAR ZERUBAVEL, associate professor of sociology at Columbia University, is the author of *Patterns of Time in Hospital Life* and of *Hidden Rhythms: Schedules and Calendars in Social Life*. He is currently involved in studying the role of the weekly cycle in social life and the role of boundaries in the social construction of discontinuity.

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DAVID L. MORGAN is currently an assistant professor in the Department of Sociology at the University of California, Riverside. His research centers on uses of social network analysis in social psychology and medical sociology. In addition to applying the techniques described in this issue to data on interracial friendships, he is studying the role that information gathered through social networks plays in the "naive epidemiology" of heart attacks.

CLIFFORD C. CLOGG is associate professor of sociology and statistics and faculty associate of the Population Issues Research Center at the Pennsylvania State University. He is the author of *Measuring Underemployment: Demographic Indicators for the United States*. His recent research

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———. 1963*b*. "The Theory of Change and Response in Modern Demographic History." *Population Index* 29: 345-66.

Goode, W. J. 1967. "The Protection of the Inept." *American Sociological Review* 32: 5-19.

Moore, Wilbert E., and Arnold S. Feldman. 1960. *Labor Commitment and Social Change in Developing Areas*. New York: Social Science Research Council.

Sanford, Nevitt, ed. 1962. *The American College*. New York: Wiley.

Weber, Max. (1921) 1968. "Society's Problems." Pp. 12-16 in *Economy and Society*, edited by Guenther Roth and Claus Wittich. New York: Bedminster.

The Standardization of Time: A Sociohistorical Perspective¹

Eviatar Zerubavel
Columbia University

In order to clarify the fundamental distinction between the psychological and sociological perspectives on temporality, this paper explores the distinctly social process of establishing a standard time-reckoning framework. The paper examines the introduction of Greenwich Mean Time to Britain, the establishment of the American railway time-zone system, and the almost universal enforcement of the modern international standard-time zone system. The rise of standard time is viewed within the context of (a) the establishment of national and international communication networks following the introduction of railway transportation and telegraphic communication (which explains the need to synchronize different communities and countries with one another) and (b) the rise of rationalism (which is responsible for the dissociation of standard time from nature). The paper also examines the various grounds on which standard time has been opposed, as well as the relevance of the time-zone system to the development of modern Western "organic" ties of interdependence and complementary differentiation.

In inventing the locomotive, Watt and Stevenson were part inventors of time. [HUXLEY 1936, p. 122]

This paper purports to shed light on the social aspects of temporal reference. While cognitive activities such as time reckoning and dating have traditionally been studied almost exclusively by philosophers and psychologists, the present discussion is an attempt to view them from a relatively novel analytical angle—the sociological perspective.

A brief look into the "temporal formulations" (Schegloff 1972, p. 116) that people use suggests a variety of ways in which the very same instant can be dated. To quote one example from fiction, "When I was a younger man—two wives ago, 250000 cigarettes ago, 3000 quarts of booze ago" (Vonnegut 1963, p. 11). This is also true of time reckoning. There are an infinite number of temporal reference frameworks within which one might

¹ I wish to thank John R. Hall, Charles Lidz, Alan Meisel, Murray Melbin, Barry Schwartz, and Yael Zerubavel for some most useful suggestions. Requests for reprints should be sent to Eviatar Zerubavel, Department of Sociology, Fayerweather Hall, Columbia University, New York, New York 10027.

anchor the present—the number of days following the purchase of a bottle of milk, the day within one's menstrual cycle, and so on indefinitely (Zerubavel 1979, pp. 103–4).

While such temporal formulations may be very useful at the personal level, they may be of no use whatsoever at the collective level. There, as Durkheim demonstrated in his pioneer attempt to clarify the fundamental distinction between the psychological and sociological perspectives on temporal reference, it is *social* time that we must adhere to: "It is not *my time* that is thus arranged; it is time in general. . . . Thus we see all the difference which exists between the group of sensations and images which serve to locate us in time, and the category of time. The first are the summary of individual experiences, which are of value only for the person who experiences them. But what the category of time expresses is a time common to the group, a social time, so to speak" (Durkheim 1965, p. 23).

It was Schutz who proceeded to fully contrast the temporal structure of purely subjective "worlds" with that of the essentially intersubjective everyday life-world (Hall 1978, pp. 47–51; Schutz 1973, pp. 221–22; 1976; Schutz and Luckmann 1973, pp. 27–36, 45–58). While not denying that individuals' "inner time" provides their own dream and fantasy worlds with an adequate structure, Schutz nevertheless maintained that it cannot possibly provide an adequate temporal structure for the everyday life-world. There, if time is to be shared as an intersubjective social reality, it ought to be *standardized*.

Standard time is thus among the most essential coordinates of intersubjective reality, one of the major parameters of the social world. Indeed, social life would probably not have been possible at all were it not for our ability to relate to time in a standard fashion:

If men . . . did not have the same conception of time . . . all contact between their minds would be impossible, and with that, all life together. [Durkheim 1965, p. 30]

The possession of means and ways to "time" the behavior of the members of any group in such a way that each member apprehends "the appointed time" in the same way as do other members has been possibly the most urgent need of social life at any time and at any place. Without this, social life itself is impossible. [Sorokin 1943, p. 173]

Social adaptation requires that time should not be the unreliable time-experience of individuals, but must be invariable and common time for all individuals on the basis of which alone cooperation in economic and social activity is possible. [Mukerjee 1943, pp. 257–58]

Anyone who has ever tried to understand from a child's description of an event precisely when it happened or how long it lasted must appreciate this necessity.

In order to understand how the *standardization of temporal reference* is

accomplished, we must appreciate the enormous significance of the distinctly human ability to calibrate subjective temporal formulations in accordance with a single standard yardstick. This presupposes the convertibility of subjective formulations of the duration of events and of their location in time into a *standard time language*, into terms which have the same meaning for others as they do for their user. Standardizing temporal reference presupposes a *standard system of units of time*, which enables different people to measure the passage of time in an identical manner, and a *standard time-reckoning and dating framework*, which allows them to date any past, present, or future instant in a common fashion.

There are a wide variety of standard ways in which people measure the passage of time even within one and the same culture. Thus, for example, we measure the duration of tennis and volleyball games in terms of sets and points, but that of musical passages in terms of measures and quarter notes. Even though they are standard, such time-measurement frameworks do not require the use of either the clock or the calendar, since none of the above units of duration is convertible into such clock-time or calendar-time units as minutes, hours, days, weeks, months, or years. In fact, for a variety of purposes, the latter are totally irrelevant. It is far more appropriate, for example, to designate the life expectancy of tires and running shoes in terms of mileage—or that of children's beds in terms of the child's weight—than in terms of years of use. It is also far more useful to measure the length of waiting lines in terms of number of people than in terms of minutes (Zerubavel 1979, pp. 90–91).

This is also true of time reckoning and dating. The standard temporal reference points that we use as "milestones" are often not anchored in either clock time, the Gregorian calendar, or the Christian Era (Roth 1963, pp. 1–62; Zerubavel 1979, pp. 88–104). Consider, for example, standard temporal formulations such as "at the time she was dating Bob," "during my senior year," "right after her operation," "when our daughter was two," and "when we were living in Chicago." Anorectic patients are usually discharged from hospitals when they reach a certain body weight, rather than according to the number of weeks they have spent there. Similarly, when discussing our car's last tune-up with our mechanic, we are more likely to temporally locate it in terms of how many miles "ago" than in terms of how many months ago.

And yet, in the modern Western world, the prevalent *standard temporal reference framework* is the one based on the use of clock time, the Gregorian calendar, and the Christian Era. As part of their primary socialization, children are taught to think about the passage of time in terms of such standard units as minutes, hours, days, weeks, months, and years—as well as to use "dates" such as "on February 15, 1914" or "at a quarter to six"—as prerequisites for their participation in the adult social world. From a modern

psychiatric standpoint, not knowing the day, the date, or the year is even regarded as indicative of some mental problem (Kantor and Lehr 1975, p. 81). And indeed, it is anxiety about being barred from participation in the intersubjective social world that accounts for the uneasy feeling that usually accompanies the realization that our watch has stopped or that we cannot recall what day it is: "Only within [the standard] temporal structure does everyday life retain for me its accent of reality. Thus in cases where I may be 'disoriented' . . . I feel an almost instinctive urge to 'reorient' myself within the temporal structure of everyday life. I look at my watch and try to recall what day it is. By those acts alone I re-enter the reality of everyday life" (Berger and Luckmann 1967, p. 28).

The need to standardize temporal reference so as to allow for a coordination of behavior exists even at the lowest level of social organization. Even the institution of the appointment would not have been possible without it. Yet the complexity of the problem is directly proportional to the size of the social system involved. It is much easier to achieve *temporal coordination* at the level of the family than at the level of the community, not to mention the city, the nation, or the world at large (Moore 1963).

Yet this is precisely what man has managed to accomplish. With the expansion of the boundaries of the social world, there has been a parallel expansion of the use of the standard temporal reference framework that prevails in it. The validity of the above-mentioned temporal reference framework has already reached the global level (Zerubavel 1981, pp. 96–100). It is possible to achieve temporal coordination today, not only at the local level, but across continents as well.

It is the purpose of this paper to continue what Durkheim and Schutz began and further investigate the social aspects of temporal reference. I shall therefore explore the process through which a major constituent of this standard temporal reference framework—namely, the international standard-time zone system—has evolved. I shall first identify the socio-historical context within which it has developed, and then examine the considerable social significance and implications of this remarkable human achievement.

GREENWICH MEAN TIME (GMT)

Compared with the Christian Era, the Gregorian calendar is a rather practical time-reckoning and dating framework, since it is sensitive to time intervals such as the month and the day, which the former is not. Yet for everyday temporal coordination in the modern world it is simply not sufficient. Social life as we know it would probably be impossible were we to rely entirely on time units at least one day long when temporally coordi-

nating ourselves with others. Making an appointment for "February 16" is definitely more practical than making it for "1982," but it is still not sufficient.

The indispensability of a standard temporal reference framework such as clock time, which involves such time units as the hour, the minute, and the second, ought to be appreciated within this context. It is far more convenient to coordinate ourselves temporally with others through clock-time formulations such as "at 4:55 P.M." than through calendar-time formulations such as "on April 27" alone.

Clock time as we know it today, however, did not come into being until 1780, when Geneva first started to use "mean time" in preference to solar time (Howse 1980, p. 82). While clocks had been in use long before that, the time they showed was quite different from the time clocks show today. In those days, clock time was a direct reflection of solar time.

Since solar time is a function of the particular position of any point on earth vis-à-vis the sun, it is not the same throughout the world. Given the rotation of the earth on its axis approximately every 24 hours, it varies by approximately four minutes for every degree of longitude. While this may be of little consequence at the level of any particular locality, it involves quite significant time differences at a supralocal level.

As late as the mid-19th century, the only valid standard of time was *local time*. Each city, town, or village had its own time, which applied to it alone. Thus, there was a plurality of local times which were not coordinated with one another, since no locality was concerned with the local times of other localities.

That situation was not even regarded as problematic until the 1840s, since the contact among communities was quite restricted anyway. Even when such contact did exist, it did not necessitate any calibration of local times, because communication certainly was not instantaneous as it has been since the invention of the telegraph and the telephone. Furthermore, minor time differences among communities were not that significant anyway, since, even when people did travel, they were usually not concerned with odd minutes. Travel in those days did not yet involve any regular services which might require precise timekeeping. This was also prior to the industrial revolution, and punctuality was not yet as culturally valued as it has been since then (Thompson 1967).

It was not until the revolution in communication that the situation began to change in any significant way. The indispensability of a uniform standard of time that would allow some temporal coordination at a supralocal level was a direct product of the establishment of a *national communication network* and was, therefore, first felt around the introduction of the British mail-coach service during the 1780s (Davies 1978, p. 194). This was the first service ever to connect different communities within one and the same

communication network, and it was, therefore, only upon its introduction that the lack of any temporal coordination among those communities first seemed to create difficulties.

The British mail-coach service also became the first public service ever to provide regularity, when it started in 1784 to run its mail coaches in accordance with *strict schedules* (Howse 1980, p. 83). Given the inherent relation between temporal regularity and punctuality (Zerubavel 1981, pp. 35-38), the mail service also came to symbolize the latter even before the rise of the factory (Wright 1968, p. 126). Having committed itself to run its mail coaches in accordance with strict schedules, the British Post Office could no longer rely on locally based practices of time reckoning. If it was to provide a regular service of mail collection and delivery among different communities, the numerous local times of those communities *had* to be coordinated with one another.

The initial push toward standardizing time reckoning on a supralocal level was thus given by the British Post Office, when it started to run all its mail coaches throughout Great Britain in accordance with a uniform standard of time (Bagwell 1968, p. 55; Wright 1968, p. 125; and an unpublished essay by Nigel Thrift, "The Diffusion of Greenwich Mean Time in Great Britain"). Since the Royal Observatory in Greenwich was the most reliable observatory in Britain, every mail-coach guard was required to carry a timepiece indicating Greenwich Mean Time (GMT), so that all clocks in the various post offices on the coach's route could be adjusted in accordance with it. *This was the first attempt in history to synchronize different communities with one another.*

And yet, only a very restricted social circle used the services of the Post Office. To anyone else, GMT was still totally irrelevant. It was not until the introduction of railway transportation, which affected a much wider population, that the need for introducing a uniform standard of time at a supralocal level became crucial.

It was railway transportation that, together with the rise of the factory, was primarily responsible for spreading the significance of punctuality and precise timekeeping among the general population (Mukerjee 1943, p. 259). It is hard to overemphasize its role in the spread of punctuality and precise timekeeping, since these are the primary imperatives underlying the worlds of the railroader (Cottrell 1939; Kemnitzer 1977) and the railroad user. Railroads operate on strict timetables, where even odd minutes count. Whereas in the past differences of a few minutes were usually inconsequential, in a railway age they might involve missing one's daily train.

The introduction of railway transportation also promoted the standardization of time reckoning at a supralocal level by bringing different communities within more immediate reach of one another, thus making people more aware of the fact that the local times of other communities were both

different from, and uncoordinated with, their own. A rise in the rapidity of ways of communication and transportation often accompanies the dissolution of segmental social structures (Durkheim 1964, pp. 259–60), and with the development of a fast railway *network* throughout Britain, communities which had previously led a rather autonomous existence gradually became interrelated parts of a single systemic whole.

That called for further coordination among them. The various communities which the railway network now connected could no longer determine their own local times in an uncoordinated fashion, independently of one another. Increasing *interdependence* among communities necessarily involves a need to achieve further temporal coordination (Hawley 1950, pp. 297–99). With the introduction of railway transportation, communities became far more dependent on one another, to the point where some temporal coordination among them became inevitable.

Modern temporal coordination relies heavily on the use of the *timetable*, and it is important to note that the railroads were primarily responsible for spreading the use of this typically modern institution (Howse 1980, p. 84). The first railroad timetables, however, were quite cumbersome, due to the lack of a single, uniform standard of time throughout the network. Those time sheets incorporated a plurality of local standards of time, and had to include footnotes converting local time into GMT (Bagwell 1974, p. 124; Robbins 1962, p. 49). Much inconvenience was involved in using a single supralocal timetable without having a single uniform standard of time.

Given all this, it is hardly surprising that the greatest push toward standardizing time reckoning at a supralocal level came from the railroad world. If there was to be a single timetable for an entire railway system, there was also a need for a *single, uniform standard of time*. It would be much more convenient if all communities that were connected by the same railway system—and which, therefore, also shared the same timetable—would also share the same standard of time.

And, indeed, it was the railroad timetable that was primarily responsible for making GMT the uniform standard of time throughout Britain. In 1840, only 15 years after the introduction of the first passenger train, Great Western Railway began to use only GMT throughout its timetables and stations, and was soon followed by other railways. With the growing use of railroad transportation, many cities soon followed the railroads' example, and by 1855, 98% of all public clocks in Britain were already set to GMT (Howse 1980, pp. 87–89, 105–14).

Had it not been through the spread of railway transportation that establishment of a single uniform standard of time became a necessity, standardization of time reckoning at a supralocal level would probably have come into being through the spread of telegraphic—and, later, telephonic—

communication. The invention of the electric telegraph—the first device ever to allow people practically instantaneous communication across long distances—clearly reduced the “temporal distance” among communities in an even more pronounced way than did the introduction of the railway network two decades earlier. It marked the first time in history that even the most distant communities could practically be in *immediate* contact with one another. That alone would have called for the establishment of a single, uniform standard of time at a supralocal level.

STANDARD-TIME ZONES

If the multiplicity of standards of time produced so much inconvenience in Britain, which, after all, covers only 8 degrees of longitude, it certainly produced much more chaos in the United States, which stretches across no less than 57 degrees of longitude (not including Alaska and Hawaii). And, indeed, by mid-19th century, practically hundreds of standards of time were in use throughout the country. To take just the Midwest, for example, there were 38 standards of time in Wisconsin, 27 in Michigan, 27 in Illinois, and 23 in Indiana (Bartky and Harrison 1979, p. 46; Holbrook 1947, p. 355; Shaw 1961, p. 35).

Given the tremendous power of the railroad companies in the United States, it should come as no surprise that the movement toward standardizing time reckoning there also originated in the railroad world. Soon after the railway system was introduced to the country, many communities adopted the time of their railroad station as their only valid standard of time. As becomes a rapidly secularizing and expanding society, the railroad station was clearly dethroning the church and the town hall as the nerve center of a standard temporal reference framework.

However, while adopting the time of the railroad station was very useful for standardizing time reckoning at a local level, it could not help temporal coordination at the supralocal level, since each community still adhered to its own local time, which was not coordinated with those of other communities. As far as railroad timetables were concerned, this was quite inconvenient. The Union Pacific Railroad, for example, had to operate its trains on no less than six different standards of time (Holbrook 1947, p. 355; Stover 1961, p. 157).

The United States is considerably larger than Britain and lacks any obvious single center like London. That precluded the adoption of a single uniform standard of time, as in Britain, and brought about the standardization of time reckoning first at the level of the railroad company. Each company came to agree on one standard of time (usually the local time of the city where its headquarters or main terminal were located) to be used on all its lines. Thus, the New York Central, for example, used New

York time throughout its entire system, while the Pennsylvania used Philadelphia time.

While things were relatively orderly at the level of each particular railroad company, the situation was quite chaotic at the level of each particular community. At the train station in Buffalo, for example, there were three different clocks indicating three different times. While one of these showed Buffalo time, the second was set on New York time and was relevant only to passengers using the New York Central trains, whereas the third was set on Columbus time and was relevant only to passengers using the Lake Shore and Michigan Southern trains (Howse 1980, p. 120; Stover 1961, p. 157; *Time* 1944, p. 86).

This involved serious practical difficulties in using the railway system effectively. Trains using different standards of time would often enter the very same station from different directions (McPherson 1907, pp. 216-17), which made it sometimes impossible to arrange for connections, since a train might be scheduled to arrive only after the connecting train had already left. A far more serious consequence than sheer inconvenience, however, was actual danger. Since terminal traffic was controlled by tight arrival and departure schedules, a difference of a minute or two could actually be the difference between a safe operation and a collision (Haines 1919, p. 387; Riegel 1926, p. 266; Shaw 1961, pp. 34-35).

Thus, in order to promote the effective use of a complex railway network as well as prevent accidents, it became necessary to standardize time reckoning at a level higher than that of the railroad company. The ground was set for a most ambitious project—the standardization of time reckoning at the level of the entire North American continent. This also entailed the introduction of a new institution—the *standard-time zone*.

The idea was originally developed by Charles F. Dowd, who proposed to divide the United States and Canada into four "time belts" which would each be 15 degrees of longitude wide. The uniform time within those time belts would be determined in accordance with four meridians whose respective distances from Greenwich would be precise multiples of 15 degrees of longitude—the 75th, 90th, 105th, and 120th meridians west of Greenwich. Since a differential of 15 degrees of longitude corresponds to a time differential of precisely one hour, all clocks throughout the United States and Canada would be adjusted in accordance with four uniform standards of time which would relate to GMT in terms of time differentials of full hours—five, six, seven, and eight hours slower, respectively (Bartky and Harrison 1979, p. 46; Haines 1919, p. 387; Holbrook 1947, pp. 355-56; Howse 1980, pp. 121-24; Kaufman 1938, pp. 70-72; Stover 1961, p. 158).

Somewhat independently, railroad executives were making their own attempts to coordinate the timetables of the various railroad companies (Haines 1919, pp. 389-90, 452; Riegel 1926, p. 267). In 1872, they orga-

nized their first Time-Table Convention, and, from 1874 on, a General Time Convention met regularly. It was from its meetings that the American Railway Association eventually came into being in 1891. That the chief American railroad organization originated in meetings held for the sole purpose of achieving temporal coordination among railroads is highly suggestive of the unique significance of time in the railroad world.

In 1881, the General Time Convention authorized its secretary, William F. Allen, to put a standard-time system into effect. Following Dowd's plan, Allen proposed to establish four sections on the basis of the 75th, 90th, 105th, and 120th meridians west of Greenwich as controlling meridians precisely one hour apart from one another (Bartky and Harrison 1979, p. 48). The convention endorsed his plan and, on November 18, 1883, Standard Railway Time was put into effect. The readjustment of clocks was scheduled for that day because it was Sunday, a nonbusiness day on which railroad traffic was relatively light and confusion and inconvenience could be kept at a minimum. (This is also why clock adjustments to and from daylight saving time now take place at 2:00 A.M. on Sunday.) On that day, four uniform standards of time were imposed on the entire American railway network—Eastern, Central, Mountain, and Pacific Standard Time.

Given the enormous power of the railroads in America around that period, the general climate was quite ripe for standardizing time reckoning at a national level. Of the 100 principal cities in the United States, 70 adopted Standard Railway Time immediately, and by October 1884 it had already been adopted by 85% of all American towns of over 10,000 inhabitants (Bartky and Harrison 1979, p. 46; Howse 1980, p. 126). The popular use of the new system of time reckoning soon transcended the railroad world and began gradually increasing in many other domains of life. At the same time, conflicts among the various railroad companies over the boundaries between time zones led them to delegate the authority for making such decisions to a single governmental agency (Vanderblue and Burgess 1924, p. 212n.). This inevitably led to centralized governmental regulation of time reckoning throughout the country, and on March 19, 1918, the U.S. Congress passed the Standard Time Act, which legalized the system introduced 35 years earlier by the railroads, and authorized the Interstate Commerce Commission (ICC) to define the boundaries between time zones (*The Statutes at Large of the United States of America* 1919, pp. 450–51). (The act also introduced a fifth standard of time, United States Standard Alaska Time, based on the mean astronomical time of the 150th meridian west of Greenwich.)

The Standard Time Act marked the beginning of a long process of defining and redefining the boundaries between time zones. In order to keep inconvenience at a minimum, the ICC was particularly considerate of the boundaries of states and counties, as well as of railroad lines and junctions.

It also seemed to acknowledge the East's cultural and commercial regional dominance. Accordingly, virtually all boundary changes made between 1919 and 1966 extended time zones westward, thus "expanding" the East. Thus, the boundary between the Eastern and Central zones was gradually pushed westward, until the western parts of Georgia, Ohio, North Carolina, and Virginia were incorporated into the former. Along the same lines, the ICC also brought the southwestern part of Kansas and the western parts of North Dakota, Oklahoma, and Texas from the Mountain zone to the Central zone, and the southern part of Idaho and the northwestern part of Arizona from the Pacific zone to the Mountain zone (Bartky and Harrison 1979, pp. 49–50; Pyne 1958, pp. 2–7; *Uniform Time* 1948; U.S. Department of Transportation 1970, pp. 15–22). (The "supremacy" of the eastern time zones is also evident from the way television networks schedule their programs! As someone commented when Americans woke up at 4:00 A.M. to watch Prince Charles's royal wedding on television, "It was a sacrifice they were proud to make, in recognition of a tradition even more sacred and venerable than the convention that all important events take place during prime time on the East Coast" ["Looking Back at '81" (1982), p. 26].)

Some confusion still remained. For one thing, since the Standard Time Act carried no provisions for its enforcement nor penalties for its violation, many communities "migrated" from one time zone to another in a totally uncoordinated fashion. Further complications were generated by the way daylight-saving time was implemented. Standardizing time reckoning on a national scale could not be possible unless the practice of advancing the time during the summer was applied in a uniform manner throughout the country. As it was, however, daylight-saving time was observed at a national level only between 1918–19 and 1942–45, when the entire nation was in a state of war and the expected saving of coal and electricity through the advancement of time was generally regarded as a desirable collective goal which could support the war effort. Otherwise, between 1920–42 and 1945–66, daylight-saving time was a matter of local option and was not adopted in any uniform manner. In 1963, for example, only 16 states observed it at the state level, 13 additional states observed it but not statewide, while the remaining 21 states did not observe it at all. Moreover, daylight-saving time was not implemented in a uniform manner even when it was observed. In 1963, for example, it was put into effect on May 26 in Colorado, on the fourth Sunday of May in Minnesota, and on Memorial Day in Montana (Bartky and Harrison 1979, pp. 49–51; *Uniform Time* 1964, pp. 37–38, 58–62; U.S. Department of Transportation 1970, pp. 6–7).

Given the incessant push toward standardizing temporal reference at a supralocal level, a reform of the Standard Time Act seemed inevitable. Its laissez-faire attitude toward observance resulted in a lack of uniformity that apparently disturbed many, and voluntary bodies such as the Committee

for Time Uniformity and the Citizens for Standard Time were established during the early 1960s (*Uniform Time* 1964, pp. 44, 131).

On April 13, 1966, the U.S. Congress finally passed the Uniform Time Act of 1966 (*United States Statutes at Large* 1967, pp. 107-9), an act far more ambitious than the 1918 act, which virtually made standard time mandatory, establishing actual penalties for failure to observe it. (It also renamed United States Standard Alaska Time as Alaska-Hawaii standard time, and established three additional standards of time—Atlantic, Yukon, and Bering—based on the mean astronomical time of the 60th, 135th, and 165th meridians west of Greenwich, respectively.) Uniform dates for the beginning and the end of the period of daylight-saving time observance were also established: the one-hour advancement of standard time is observed from 2:00 A.M. on the last Sunday in April until 2:00 A.M. of the last Sunday in October every year. On April 1, 1967, the day on which the Act became effective, the Department of Transportation was created. As if to symbolically suggest once again the centrality of transportation to the need to standardize time reckoning on a national scale, the Secretary of the new department immediately assumed responsibility for all matters concerning standard time throughout the United States.

All in all, the American attempt to achieve temporal coordination on a national scale has been extremely successful. As we shall now see, it also gave the major push toward the far more ambitious attempt to achieve temporal coordination at a global level by establishing a system of standard-time zones throughout the entire world.

AN INTERNATIONAL STANDARD-TIME ZONE SYSTEM

The problems involved in the lack of uniform standards of time at the international level first became apparent with the advent of the European railway network, but the pressure toward standardizing time reckoning at that level built up especially during the 1870s, following the laying of the Atlantic cable which connected Europe and America telegraphically, and with the establishment of a meteorological bureau and a signal-service system (Howse 1980, pp. 117-20). The idea of dividing the entire world into 24 time zones which would be 15 degrees of longitude wide and at one-hour differentials from one another was promoted, from 1876 on, by Sandford Fleming, the chief engineer of the Canadian Pacific Railway.

In 1882, responding to petitions laid before it by the American geographical and meteorological societies, the U.S. Congress authorized President Chester Arthur to organize an international congress for the purpose of establishing a universal standard-time system within the domains of both science and commerce (*To Fix a Common Prime Meridian* 1882). The president proceeded to invite all governments that held diplomatic

relations with the United States to participate in the International Meridian Conference, which was to be sponsored by the secretary of state and held in the diplomatic hall of the State Department in Washington, D.C., during October 1884.

In all, 25 countries accepted the invitation and registered as participants. In line with the pronouncedly "rationalistic" spirit of the period, most of their delegations consisted of professional "experts" such as directors of national observatories, representatives of hydrographic services, and top naval personnel. A number of leading scholars who were in Washington at the time were also invited to present their views before the delegates.

The *Proceedings of the International Meridian Conference* (1884) are particularly useful to the sociologist, since they display the actual process by which international standard time was socially constructed. The debates around the resolutions of the congress serve to remind us that temporal reference frameworks do not only reflect the rhythm of social activity (Durkheim 1965, pp. 23, 32n., 391, 488), but are also the product of actual negotiation processes.

The issue of standard time was connected to that of establishing a uniform initial meridian, and it was quite obvious that the first task of the conference would be to choose a prime meridian. The first meridian to be proposed was the one passing through the center of the transit instrument of the Greenwich Observatory. Indeed, most delegates must have regarded the Greenwich meridian as the obvious choice. After all, it was within the domain of navigation that the need for a common prime meridian was most strongly felt, and Britain was clearly the world's principal maritime power. Since it provided most of the world's admiralty charts and nautical almanacs, no less than 72% of all the world's floating commerce was carried out with navigators determining longitude as well as time in accordance with Greenwich. From the 18th century on, navigators around the world used to set their chronometers by GMT, and, since the publication of the British *Nautical Almanac* in 1767, Greenwich was used as the prime meridian on maps and charts of most nations (Howse 1980, pp. 72, 129-31). Furthermore, the most extensively used system of railroad transportation in the world—the American and Canadian railway network—had already adopted a standard time-reckoning system essentially derivative of GMT. Both American and British delegates stressed time and again that choosing the Greenwich meridian as the world's prime meridian would involve the least number of necessary alterations of the system already in use, and that choosing any other meridian would only add unnecessary confusion, expense, and inconvenience (*Proceedings of the International Meridian Conference* 1884, pp. 23-25, 45, 55-58).

The main opposition to Greenwich came from Britain's traditional arch-rival, France. That Paris and London are only slightly more than 2 degrees

of longitude apart from one another only served to highlight the centrality of the symbolic element built into the decision regarding the location of the prime meridian. Much of the debate that took place was, therefore, extremely hot.

The French defied the supremacy of the Greenwich meridian by emphasizing the principle of neutrality (*Proceedings of the International Meridian Conference* 1884, pp. 22, 27, 38). In order to appear as representing universal ideals rather than particularistic French interests, they formulated most of their objections to Greenwich in general arguments, and only on very few occasions did they even mention it by name. In order to convince the other delegates that the problem lay not with Greenwich in particular, but with any place that was not "neutral," they were even willing to rule out Paris.

Their main opposition came from the American and British delegations. They, too, denied any particularistic national interests, advocating, instead, "the common good of mankind," and calling all delegates to be "citizens of the world." Stressing the necessity of relinquishing any national pride, the American delegation also emphasized that it was they, rather than the British, that had proposed Greenwich (*Proceedings of the International Meridian Conference* 1884, pp. 7, 25, 44). However, the strategy chosen by the British and American delegates to display their universalistic orientation was quite different from that of the French. Rather than present themselves as being guided by neutrality, they claimed that the main principles underlying their decisions were practicality and convenience (*Proceedings of the International Meridian Conference* 1884, pp. 24, 31-32, 36-37, 39-40, 55-56, 63, 72-73, 78).

The French made some desperate attempts to dissuade other delegates from voting for the American resolution. At their request, the conference even interrupted its meetings for an entire week, until a stenographer was found who could translate all their speeches into English, to make them available to all delegates before the vote. Yet they fought a losing battle right from the start, and the decisive majority of the delegates voted for Greenwich.

After having settled on the Greenwich meridian as the world's prime meridian, the conference proceeded to establish an international system of time reckoning based on it. It agreed that the multitude of actual solar times that applied only to particular communities had to be replaced by a small number of "mean local times," each of which would apply to an entire region. The main problem was where to introduce the essentially artificial breaks that would transform the continuum of actual solar times into a series of discrete standards of social time.

A proposal to set those breaks at 10-minute intervals from one another (*Proceedings of the International Meridian Conference* 1884, pp. 106-10)

allowed for a close approximation of the mean standard time of any particular point on earth to its actual solar time, yet it would have introduced much inconvenience by creating no less than 144 time zones. And here again it was practicality and convenience that seemed to have guided the conference. Rather than divide the world into 144 10-minute-wide time zones, it adopted a system quite like the one introduced a year earlier by the North American railroads. Though the conference never passed any actual resolutions regarding such a system, it generally agreed that the world would be divided into 24 one-hour time zones.

To complete the establishment of an international time-reckoning system, the conference also passed a resolution regarding a standard beginning of the daily cycle. In those days, while civil days were reckoned from one midnight to the next, astronomers and navigators counted days from one noon to the next. In order to avoid situations involving a duality of dates during most business hours (Howse 1980, p. 149), the conference rejected the astronomical and nautical day and established that the "universal day" would begin throughout the world at midnight (*Proceedings of the International Meridian Conference* 1884, p. 59).

However, one problem still remained. Given the eastward direction of the rotation of the earth, eastern points ought to precede western points in time. Yet, given the sphericity of the earth, "east" and "west" are relative concepts. After all, if I leave Richmond, Virginia, and keep moving eastward, I shall eventually arrive at San Francisco, which evidently lies to the *west* of my point of departure. This has some very interesting implications for time reckoning. It is not at all clear, for example, whether Richmond lies three hours "ahead" of San Francisco or 21 hours "behind" it. This is by no means a mere academic matter, at least as far as determining the date is concerned. If it is 5:00 P.M. on Tuesday in Richmond, a decision ought to be made as to whether it is 2:00 P.M. on Tuesday or on Wednesday in San Francisco!

Prior to 1884, some confusion regarding dates did exist at the international level:

Thus there was a discrepancy as to date around the borders of the Pacific according to whether the colonizers came from the east or the west. The Portuguese, then the Dutch, the French, and the British, came to the East Indies by way of the Cape of Good Hope. The Spaniards, on the other hand, reached the Philippines and Ladrones from America. Until 1844 the Philippines kept "American date" while Celebes, in the same longitude, kept "Asiatic date".

To the north there was another example of "reaching into the wrong hemisphere", this time with Asia reaching into America. Inspired by Bering's discoveries, Russian fur-traders settled in Alaska as early as 1745. In course of time Alaska became a Russian colony, in which the Orthodox Church, with its Julian Calendar, was established, the dates being the same as those in St. Petersburg and Moscow. In the nineteenth century American

traders streamed into Alaska causing worry to the Orthodox priests, not only by their adherence to the Gregorian calendar (which affected the day of the month) but also because they insisted upon observing their day of rest on the day that the Russians claimed to be a Monday. [Howse 1980, p. 162]

In its attempts to establish an international time-reckoning system, the conference certainly had to deal with such problems. A decision had to be made as to what constitutes "east" and what constitutes "west," that is, which points on earth would be regarded as "preceding" or "following" others, as far as time reckoning is concerned. Any such decision would inevitably be arbitrary, but if any temporal coordination at a global level was to be achieved, deciding on a standard cut-off point where the date would change required full international consensus.

The conference decided on a standard meridian on which the date would change. Consequently, to this day, as far as time reckoning is concerned, this "International Date Line" is regarded as the easternmost meridian on the globe (as if the earth were, indeed, flat), so that all points lying east of it are regarded as lying west of it. Thus, if it is Tuesday evening in Tokyo, it is still Tuesday morning—rather than Wednesday morning—in New York, since New York is regarded as lying 14 hours behind Tokyo, rather than 10 hours ahead of it.

The temporal relation between New York and Tokyo is a function of where the conference decided to fix the International Date Line. For the sake of convenience, it was fixed on the 180th meridian—both east and west of Greenwich—which happens to run across an almost entirely uninhabited area. To this day, travelers who cross this line on their way eastward "gain" one day and must move back from Wednesday to Tuesday, while those who cross it going westward "lose" a day and must advance their calendars from Wednesday to Thursday.

Despite the fact that all delegates had been authorized to represent their governments, countries were not obligated to implement any of the resolutions passed by the conference (*Proceedings of the International Meridian Conference* 1884, pp. 65–66). And, indeed, as we have seen already in the case of the hostess country itself, not every country that participated in the conference was ready to accept its resolutions right away and abandon all local time-reckoning practices for the sake of synchronizing itself with the rest of the world.

The movement toward establishing a single time-reckoning system throughout the entire world has been gradual (Gould 1932, pp. 2–3, 8–17; Harrison 1935, pp. 166–70; Howse 1980, pp. 154–55; Strahler 1975, p. 80; Strong 1935, p. 482). While some countries adopted standard time right after the International Meridian Conference, it was only in 1940 that Hol-

land, for example, synchronized itself with the rest of the world. At the present moment, however, only a very few countries still do not adhere to the universal time-reckoning system that prevails throughout the world. (For the current world scene, see Markowitz [1978], p. 320; *Nautical Almanac for the Year 1979*, pp. 262-65; Rand McNally [1974], p. 305; *Worldmark Encyclopaedia* [1976].)

Not all countries have committed themselves to using standards of time that are at differentials of complete hours from one another. Australia (in the Northern Territory and in Southern Australia), Canada (in Newfoundland), India, and Malaysia (in Malaya) are some notable examples of countries adhering to standard times that are several hours and 30 minutes faster or slower than GMT. There are also countries and territories such as Guyana and the Chatham Islands that adhere to standards of time that are several hours and 15 minutes faster or slower than GMT.

Despite the general success of the introduction of the standard-time zone system, some confusion still exists regarding the practice of advancing the time during the summer. As far as daylight-saving time is concerned, international coordination is still at a very primitive stage. Not all countries observe it, and even those that do vary with respect to the precise period of the year during which they do. This hinders much of the otherwise smooth temporal coordination among countries. When making a telephone call or a reservation for a flight from one country to another, it is not sufficient that the official time differential between the two countries be taken into account. One must also consider whether either of the countries, or both, observe daylight-saving time during that particular time of the year.

In the United States, this problem exists even at the national level. The Uniform Time Act allows states—and, since 1972, even parts of states that are split by zone boundaries—to exempt themselves from observing daylight-saving time, an option that has already been used by Arizona, Hawaii, and eastern Indiana. This obviously complicates the situation and introduces further confusion. If I call someone in Arizona at 10:00 A.M. Eastern Time, I must remember not only that that corresponds to 8:00 A.M. Mountain Time, but also that, from May through October, it is only 7:00 there.

THE OPPOSITION TO STANDARD TIME

Standard time is still not observed by everyone. First, there are various sectors within society that even today avoid using any mechanical timepiece whatsoever (Horton 1967, pp. 8-10). Furthermore, even at the level of one and the same organizational complex, actual standardization of temporal reference is not yet always officially enforced (Zerubavel 1979, pp. 95-97). Yet, beyond mere indifference, standard time has also met with a consid-

erable element of actual hostility (Holbrook 1947, pp. 356–59; Howse 1980, pp. 106–13).

Some of the opposition toward its introduction came on purely practical grounds, mainly from communities whose local times differed materially from the standards they were to adopt. Note that, in communities located as far as $7\frac{1}{2}$ degrees of longitude away from the meridian that controls their time zone, standard time might differ by as much as 30 minutes from actual solar time. This involves considerable shifts in the clock times at which the sun rises, and, consequently, at which people must get up in the morning (an issue that has also been central to the entire debate concerning the introduction of daylight-saving time).

However, the controversy has also had some purely extrapractical, symbolic overtones. The introduction of standard time—like that of daylight-saving time—has been viewed as a blasphemous interference with the divine natural order. Even today, despite the fact that most Moslem countries, for example, have officially adopted international standard time, they still schedule such events as prayers in accordance with solar time (Meziane 1976, p. 221; *Worldmark Encyclopaedia* 1976, passim).

Furthermore, as is quite evident from history, time may also be used for expressing particularistic—sometimes even separatist—sentiments. Note, for example, the deliberate attempts made by one utopian communal group to symbolically separate itself from the American society at large by setting all its clocks one hour ahead of “outside time” (Hall 1978, p. 55). Consider also the calendrical conflicts between the Sadducean and Pharisaic parties during the period of the Second Temple, the calendar of the Dead Sea Sect, the introduction of Easter, and the Mohammedan and French Republican calendrical reforms (Zerubavel 1981, pp. 75–95; Zerubavel 1982). The fact that, for two centuries after January 1 was officially established as the standard beginning of the calendar year, both Pisa and Venice still clung to their traditional practices of beginning the year on March 25 and March 1, respectively (Boyle 1967, p. 675; Poole 1921–23, pp. 118, 124), also ought to be appreciated within this context.

A group's effort to accentuate its separateness from other groups by refusing to be temporally coordinated with them may be seen in the attempt of some societies to resist the pressure to give up local time-reckoning practices and adopt standards of time that would synchronize them with the rest of the world. Only a few years ago, Liberia, for example, still adhered to a standard of time that was 44 minutes slower than GMT, whereas Afghanistan's standard time was 4 hours and 26 minutes faster than the latter. Furthermore, in such countries as France, Libya, Mexico, Nicaragua, Spain, and Vietnam, the official standard time is by no means the same as the “legal time” that prevails in the country (*Nautical Almanac for the Year* 1979, pp. 262–65). (This phenomenon is significantly dif-

ferent from the one discussed earlier, namely, adhering to standards of time several hours and 30 minutes faster or slower than GMT. Half hours are essentially rounded-off units of time, so that a 5-hour-30-minute differential from GMT is indicative of at least some attempt to be synchronized with the rest of the world.)

While resisting the international time-reckoning system may be viewed in some cases as mere indifference to it, there are at least some other cases where it clearly cannot be seen as anything other than a symbolic act of actual defiance. Note, for example, the defiant tenor of the following statement made by the Ayatollah Khomeini: "The heads of our [Muslim] countries are so influenced by the West that they have set their clocks according to European time. It's a nightmare" ("The Khomeini Enigma," 1979, p. 28). Such an attitude is clearly indicative of a quest to defy standardization and universalism and preserve particularistic sentiments by maintaining uniquely distinct sociotemporal arrangements.

FROM NATURAL TIME TO RATIONAL TIME

The abolition of local time-reckoning practices and the introduction of supralocal standards of time mark a most significant point in the history of man's relation to time, namely, the transition from a naturally based manner of time reckoning to a socially based one. Since we no longer set our clocks by the sun, the time they indicate is no longer derived directly from nature. With the exception of a single meridian within each time zone, there is always at least some discrepancy between standard clock time and actual solar time. In dissociating the former from the latter, we have removed ourselves one step further away from nature.

To appreciate the *artificial* nature of standard time, consider the use of one-hour differentials between neighboring time zones. While these certainly facilitate the conversion of times from one zone to another, they make very little sense from a purely natural standpoint. Whereas solar-time differentials among communities essentially progress in a continuous fashion, standard-time differentials create clock-time *discontinuities*. The abrupt one-hour differential that zone boundaries sometimes create between communities that are within walking distance of one another is totally unjustifiable from a purely physiotemporal standpoint.

Another reason clock-time differentials do not necessarily correspond to actual solar-time differentials is that not all time-zone boundaries are fixed at precisely 15 degrees of longitude from one another. That each country makes its own decision regarding the standard of time to which it adheres creates many situations that are quite understandable from a political standpoint yet are rather awkward from a purely geographical standpoint.

The western part of Tibet, for example, lies west of Nepal and is therefore about half an hour behind it in actual solar time. And yet, for various political reasons, the standard time to which Tibet adheres is two and a half hours *faster* than the one to which Nepal adheres! Note also the 24-hour clock-time differentials that exist even within one and the same group of islands, the Marshalls, which straddle the International Date Line.

What seems to have replaced nature as a temporal referencing anchor is the principle of rationality, long viewed as one of the key characteristics of modern civilization (Durkheim 1964, pp. 289–91; Simmel 1964, pp. 130–37, 189–94; 1978, pp. 429–46; Weber 1958, pp. 13–31). Indeed, we can identify a rather consistent trend toward rationalization of temporal reference during the last few centuries. Consider, for example, the rise of precise timekeeping as a significant symbolic representation of rationalism (Simmel 1950, pp. 412–13). Note also the high representation of men of science at the International Meridian Conference, as well as the central role they have played in modern reforms concerning temporal reference, a typical example of which is the pronouncedly rationalistic French Republican calendrical reform (Zerubavel 1981, pp. 88–91).

To further appreciate the “rational” character of standard time, note also how the various standards of time around the world—just like the units of time we use (Zerubavel 1981, pp. 59–61)—are neatly interrelated in mathematical terms. The mathematical thrust of the time-zone system is quite evident from the fact that the time differentials among the various zones are designated in terms of “rounded off” units of time such as the *hour*, a classic example of a purely artificial mathematical invention. (It is also by a full hour that we advance our clocks when observing daylight-saving time.)

Since most of the world is committed to the use of standards of time that are at differentials of full hours from one another, time differentials between almost any two places on earth are precise multiples of the hour. Consequently, almost any time conversion from one locality to another—even if the two are located in two distant continents—involves no more than the fairly simple adjustment of adding or subtracting a number of full hours. After all, when it is 9:27 in London, it is precisely 4:27 in New York.

The standard-time zone system has clearly contributed to the development of social solidarity by promoting *interdependence* at both societal and global levels. Since it was introduced, people no longer reckon the time of day independently of one another, because the various standards of time to which they adhere are derivative from one and the same time-reckoning *system*. The conspicuous interrelatedness of 4:27 and 9:27 results from the fact that the time zones within which New York and London are located are both parts of one and the same time-zone system. As far as temporal coordination is concerned, there is more interdependence between New

York and London today—despite the distance between them—than there used to be between New York and Philadelphia only 100 years ago.

But it is specifically *organic* solidarity that the time-zone system has promoted, by creating complementary differentiation, undoubtedly the fundamental principle underlying this type of social solidarity (Dürkheim 1964). Note that the tendency to apply a single standard of time throughout the entire country even when that country is exceptionally wide is to be found only in societies that emphasize *mechanical* solidarity and uniformity and strive toward political centralization. The Soviet Union, for example, is divided into no fewer than 11 time zones, yet all Soviet airports and railroad stations must observe Moscow time (Priestley 1968, p. 43). Similarly, even though it is nearly 50 degrees of longitude—that is, more than three solar hours—wide, almost the entire area of mainland China is placed within one and the same time zone. In marked contrast to such extreme manifestations of mechanical solidarity, it is particularly noteworthy that at no time has the United States, which evidently stresses decentralization, ever seriously considered the possibility of adopting a single standard of time throughout the entire country.

Thus, the standard-time zone system—which essentially involves a plurality of standards of time—does more than just allow at least some approximation of clock time to actual solar time (contrast here the United States with Tibet). Given that temporal coordination serves to solidify “organic” ties among people (Zerubavel 1979, pp. 60–83; Zerubavel 1981, pp. 67–69), this system also seems to be a perfect manifestation of the modern prevalence in the West of interdependence and complementary differentiation over “mechanical” ties of similitude and uniformity, and is thus one of the key characteristics of modern Western civilization.

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Community Change and Patterns of Delinquency¹

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One of the most important findings of the classic Shaw and McKay delinquency research was that the distributional pattern of delinquency in Chicago remained relatively stable over time despite processes of ethnic and racial invasion and succession. This paper re-examines this proposal during three 10-year periods spanning 1940–70 using Shaw and McKay's own data as well as a more recent set of observations. Evidence is presented that their model is tenable only between 1940 and 1950; since 1950, changing neighborhoods tend to be characterized by changing levels of delinquency. Historical shifts in the nature of ecological processes that may account for this divergence are discussed.

One of the firmly established traditions in American criminology is the investigation of spatial distributions of crime and/or delinquency in a community, reflecting the more general sociological interest in the adaptation of social groups to processes of urbanization and changing forms of social organization. Within the United States, Clifford Shaw and Henry McKay are considered to have pioneered this approach (see Shaw 1929; Shaw and McKay 1931, 1942, 1969), providing the empirical and theoretical standards that have guided many subsequent large-scale studies.

Shaw and McKay's findings are not easily condensed into a few simple statements, for these researchers were sensitive to the very complex nature of the delinquency process. Thus, as Kornhauser notes (1978), a careful reader will find knowledge of such diverse factors as socioeconomic status, social disorganization, and cultural conflict underlying their interpretations of the geographical distributions presented in their research. However, one proposition has led to what James Short has called "one of the most publicized controversies concerning their work" (1969, p. xxviii). Generally stated, Shaw and McKay presented evidence that, regardless of changes in

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the ethnic or racial makeup of a community, the relative distributional pattern of delinquency throughout the city of Chicago remained fairly stable over time. This finding was central to their work and resulted in the formulation of their famous cultural transmission hypothesis. In this paper, the stability of these delinquency patterns will be reexamined, and an effort will be made to determine whether the Shaw and McKay model has remained viable in light of the important social changes that have occurred since the publication of their research.

SPATIAL DISTRIBUTIONS OF DELINQUENCY AND HUMAN ECOLOGY

Shaw and McKay never claimed that they were the first to investigate the geographical distributions of juvenile delinquency. In their introduction to the 1942 volume, they cite not only the spatial work of European criminologists (especially in France and England; see Morris [1957] or Phillips [1972]) but also the American research of Breckenridge and Abbott (1912), Blackmar and Burgess (1917), and McKenzie (1923) that preceded their first major report in 1929. However, Shaw and McKay were not satisfied with the descriptive emphasis found in these studies and sought to interpret the spatial distributions within a general macroscopic theory of community processes. It was this important empirical/theoretical synthesis that gave the Shaw and McKay research its significance. Broadly stated, they proposed that the spatial distribution of delinquency in a city was a product of "larger economic and social processes characterizing the history and growth of the city and of the local communities which comprise it" (1942, p. 14).²

This orientation was a direct extension of the ecological perspective on community processes that had been developing at the University of Chicago under the guidance of Robert Park and Ernest Burgess. Park and Burgess were deeply concerned about the effects of growing urbanization on patterns of social organization and guided their students in a series of detailed empirical studies of urban life in Chicago. Even today, the monographs resulting from this work (see Thrasher 1927; Anderson 1923; Wirth 1928; Zorbaugh 1929) are considered classic examples of insightful and innovative research. Despite the diversity of the urban phenomena upon which they focused, these studies all reflected the human-ecology framework introduced into urban sociology by Park and Burgess (1921), a framework that interpreted the spatial and temporal development of local areas of Chicago within larger processes of urban growth, competition, specialization, and composition. This perspective was formally de-

² In the following pages, the citations to the 1942 Shaw and McKay monograph are taken from the edition reissued in 1969 because of its wider availability. Changes between the two editions will be noted.

veloped in the 1925 Burgess paper entitled "The Growth of the City" in which his now-famous concentric zone theory of urban development was first published.

As Hawley has argued (1950, p. 400), at the heart of classical human ecology is the proposition that urban growth not only physically enlarges the city but also results in a sequence of redistributions in which one type of land use or population type replaces that originally found in an area. Burgess proposed that this growth process could be modeled by a series of circular zones, centered around the midpoint of a city, that expanded their area by the invasion and succession of the next outer zone, resulting in a relocation of individuals and groups (1925, pp. 50, 54). Guest and Weed (1976, p. 1088) point out that the Chicago school of ecology felt that this relocation resulted primarily from increases in a group's economic and social status in the city. That is, as groups assimilated themselves more fully into the economic and occupational structure of a city, they would redistribute themselves by "invading" the next outward zone (which was more socially desirable) and gradually replace the groups residing there.

Shaw and McKay felt that the dynamics of this process gave a local community a "character which differentiated it from other communities . . . [that] becomes embodied in the institutions and groupings of the community" (Shaw 1929, pp. 4-6). This "character," they proposed, persisted in an area and was assimilated into the social life of whatever group was currently dominant. The basic evidence they offered in support of this position was the finding that city-wide patterns of delinquency remained fairly stable despite the shifting of population groups from one local area to another.

The validity of the cultural transmission hypothesis that they inferred from this finding is questionable, since their data contained only very indirect measures of community "culture" (see Jonassen 1949; Kornhauser 1978; Kapsis 1978). However, this does not detract in any way from the importance of their findings concerning the relative stability of delinquency patterns in the face of neighborhood change and the basic assumption that spatial distributions of delinquency could not be understood apart from the dynamics of population redistribution implied by urban growth. Despite this key assumption, dynamic considerations have been incorporated into very few ecological studies of delinquency.³ For example, although Dunn's recent (1980) review of spatial approaches to crime emphasizes that such early researchers as Shaw and McKay were most interested in the urban processes underlying geographical distributions,

³ The discussion centers only on areal studies of one (or several) communities. Other investigators have focused their attention on the distribution of crime in larger aggregates, such as the entire United States.

studies with an explicitly dynamic focus cannot be found cited in his fairly comprehensive survey of research. Thus, such important studies as those of Allison (1972), Beasley and Antunes (1974), Boggs (1965), Bordua (1958-59), Chilton (1964), Pyle et al. (1974), and Quinney (1964) should be regarded most correctly as ecological studies that present the correlates of a geographical distribution of delinquency at one point in time rather than investigate the dynamic processes underlying such distributions. One study that did approach a true replication of the Shaw and McKay research was Schmid's (1960*b*) analysis of delinquency rates in Seattle during the periods 1939-41 and 1940-50 in which he reached conclusions similar to theirs regarding invasion and succession. This is the only published study that we are aware of that approaches comparability with the Shaw and McKay research.

The scarcity of dynamic spatial investigations of delinquency is not due to a lack of sensitivity to such issues by ecological researchers. Shaw and McKay had access to a unique set of delinquency data spanning several decades of juvenile court referrals. Some investigators who have incorporated dynamic considerations into ecological models have been forced to analyze only changes in census indicators of a community, such as population size or racial composition (see, e.g., Lander 1954; Schmid 1960*a*; Kapsis 1978). Unfortunately, data concerning related changes in delinquency rates for the same periods are not usually available in most communities, making a full investigation of the dynamics of delinquency impossible.

However, the crucial difference between *static* and *dynamic* spatial approaches to delinquency is often ignored and can make comparisons of the ensuing findings very misleading.⁴ Since urban dynamics are centrally implicated in the ecological analysis of spatial distributions, it is imperative that, if cross-sectional data are used as a base of generalization, researchers must appreciate the ever-changing structure of the city. Hunter (1974), for example, has presented evidence that the associations between certain indicators of family status, economic status, and racial composition have changed in Chicago between 1930 and 1960. Over this 30-year period, the relationships among the variables became weaker and the city's local communities were becoming more ecologically differentiated. Winsborough (1961) has also shown that the distribution of population density does not remain constant over time: in the early stages of a city's life, population is much more concentrated at the center than it is in later stages. Since these variables are typically used in ecological analyses of delinquency, it seems crucial to be cognizant of such dynamics before cross-community findings are compared.

⁴ Haggerty (1971) has illustrated the pitfalls in confusing these two aspects of Burgess's ecological model, which he considers to be unquestionably dynamic.

THE ECOLOGICAL ANALYSIS OF SHAW AND MCKAY

The 1929, 1931, and 1942 publications of Shaw and McKay focused on the juvenile-court referral rates of Chicago adolescent males in the periods 1900–1906, 1917–23, and 1927–33. Later work by McKay (1969) expanded the series to include 1934–40, 1945–51, 1958–61, and 1962–65, resulting in a remarkable 65-year period of analysis.

Shaw and McKay were very meticulous and innovative in the derivation of their findings. They not only painstakingly mapped the address of each offender onto outline maps of Chicago but also computed multiple linear and curvilinear regressions by hand. However, they repeatedly cautioned the reader concerning problems inherent in their statistical methodology. For example, they noted that a random component is an element of any measurement of change based on rates, that the level of aggregation affects the magnitude of correlations, that aggregate correlations cannot be typically generalized to the individual level, and that official records are less than ideal for the study of delinquency. In addition, they stressed that they were dealing with *general* trends, noting that within any single area of the city, there was variation in rates and that certain areas of Chicago (such as the Gold Coast) did not fit the model at all.

Unfortunately, they were hindered in their analysis by technological limitations. The 1929, 1931, and 1942 monographs were researched well before computers were widely available, and thus all calculations had to be made by hand. Their conclusions concerning the invasion of new population groups into an area were primarily based on a visual inspection of the mapped distributions, although some use of correlation coefficients was made. Therefore, except in the most general sense, it is impossible to determine the extent to which variations from this pattern weaken their overall findings. Since the invasion and succession of new demographic groups is at the heart of their ecological approach to delinquency, it is important to understand these dynamics in as great detail as possible. In this paper, we attempt to move from the somewhat impressionistic analysis of Shaw and McKay to a more rigorous statistical treatment of invasion, succession, and delinquency.

THE DATA

Through an extremely fortunate stroke of luck, many of the Chicago juvenile-court referral data used by Shaw and McKay in their analyses were located in several file cabinets in the basement of the Institute for Juvenile Research, along with fairly complete documentation by McKay. In addition, the institute had collected a similar data set in 1970. Therefore, it is possible both to refine their ecological approach to delinquency by applying newer techniques of analysis to the same sets of data used in the

original influential investigations and to expand these data with a new series of observations.

Community-specific rates for these data were already detailed through 1965 in a series of internal office memos prepared by McKay. However, we feel that the original data are necessary for the analysis because it is desirable to have the rates based on consistent periods of time rather than on the seven-, four-, and one-year periods referred to in these memos. A further consideration is that the original memos cover different time segments, some straddling the census year (such as 1945–51) and others ending with it (such as 1934–40). Since changes in community characteristics are central to this analysis, and since the census data necessary for such an analysis are most reliable as indicators of these characteristics during the year in which they are collected, it seemed unwarranted to assume that the characteristics remained constant during the entire time periods under consideration. Therefore, we considered for the analysis only referrals of males to the Chicago juvenile court that occurred during census years (i.e., 1940, 1950, 1960, 1970). These referrals were used to construct the rates of delinquency per 1,000 male adolescents between the ages of 10 and 17.

Several problems in the reconstruction of these data naturally occurred. The most serious was that the 1960 data pertaining to five areas of Chicago had disappeared from the files. The rates for these areas were estimated from the memos of McKay for the period 1958–61 by assuming that the rate remained constant during this period and using one-fourth of the total number of referrals in the area for this period as the estimate of the numerator in the construction of the rate.

The reader should also be aware that the spatial units of analysis in this paper are somewhat different from the 140 square-mile areas used by Shaw and McKay. Since there is no surviving documentation of the boundaries of these areas, reconstruction of census materials is impossible. As an alternate level of aggregation, the 75 recognized local community areas of Chicago have been used (see Hunter 1974). McKay himself (1969) used these spatial units in his final analysis of these rates. One community area—number 32, the Loop—had so few males aged 10–17 that its rates were highly unstable; this area was excluded from the analysis, resulting in 74 areas to be analyzed per time period.

A final complication is that the data collected prior to 1934 are in extremely rough shape, and some of their essential documentation has not yet surfaced. Therefore, the analysis to follow is based on the data of 1940, 1950, 1960, and 1970.

These data are, of course, official records and subject to all of the criticisms aimed at data of this type. Jensen and Rojek (1980, p. 81) have summarized these criticisms, emphasizing that such data reflect several

other factors in addition to the committing of a delinquent act. These can include police/youth interaction, the offense history of the youth, and so forth. DeFleur (1975) argues that all the changes in the spatial distribution of Chicago drug arrests between 1942 and 1970 were due to systematic changes in law-enforcement procedures rather than to actual changes in the distribution of delinquent behavior, although she did not indicate whether these changes in the actions of the police were directed against certain communities or just against certain offenses. Such criticisms have important implications for studies of this type.

We certainly cannot claim that our data are an accurate reflection of all delinquency, since the self-report/official record controversy of the last several decades has emphasized that the technique of data collection is an important correlate of the findings that emerge. However, Hindelang, Hirschi, and Weis (1981) and Elliott and Ageton (1980) have shown that official records reflect distributions very similar to those found in self-reported studies when more serious offenses are compared and bases of measurement between the two techniques are standardized. Therefore, we will consider these data to be approximate representations of the spatial distributions of serious delinquency in Chicago.

In addition, Shaw and McKay did recognize the possibility of shifts in the emphasis of police priorities and legal definitions and therefore downplayed the role of absolute changes in delinquency and emphasized the relative structure of change throughout Chicago. Similarly, in this study, changes in communities are only considered relative to one another at the same historical time. By approaching the analysis in this manner, all communities are considered to be subject to the same changes in police priorities, assuming that changes in police bias toward specific communities are negligible during the period of study.

THE ANALYSIS OF CHANGE

In this analysis, four demographic variables are used as indicators of the population redistribution processes implied by the invasion/succession concept (see Burgess 1925, pp. 47-62): (1) changes over 10 years in the population size of a community (*P*), (2) the percentage of foreign-born whites (*FB*), (3) the percentage of nonwhites (*NW*), and (4) levels of household density (*DN*—the percentage of households with over one person per room).⁵ A fifth variable, the delinquency indicator (*D*), is the

⁵ At first glance, the inclusion of a household-density measure may not seem as intuitively obvious as the other indicators of ecological change. However, after 1940, there was little construction of moderate to low-priced dwellings in Chicago (other than public housing). Therefore, it has been noted that neighborhoods undergoing change at this time were often characterized by a crowding into the available living space (see Abrahamson 1959).

change in the delinquency rate per 1,000 males aged 10–17 during those 10 years.

Since the emphasis is on the dynamics of community change, the operationalization of this concept is a crucial consideration. One possible approach is the creation of a relative-change score for each variable, that is, the ratio of change in the indicator during 10 years to the initial level of that indicator, thus creating a measure of proportional change. However, such scores have the unfortunate drawback of being artificially inflated when the denominator is small and are undefined when the indicator during the initial period is zero (an ever-present possibility in delinquency rates).

In place of a relative-change measure, a residual-change score has been used, as suggested by Bohrnstedt (1969) and previously used in delinquency research by Elliott and Voss (1974). To derive this measure, the level of a variable at time t is regressed on its level in the preceding period (e.g., $D_{50} = f[D_{40}]$). The equation is then used to predict the level for each community at time t . This score is subtracted from the observed level at time t , resulting in the residual-change score. This procedure is used to compute the measure of change for each of the five variables in the analysis.

The residual-change score has several desirable properties. Since the initial level of the variable is used to predict the expected level 10 years later, the change scores reflect these levels. However, since the distribution of the change scores reflects the portion of the variance of the indicator at time t that is unrelated to the level of the indicator at $t - 10$ years, these scores are statistically independent of the initial levels of the variable. Therefore, they represent the change in the indicator levels that is not expected on the basis of the initial level alone. This is an important advantage from the viewpoint of Shaw and McKay's thesis, since it statistically controls for the increase or decrease in the levels of delinquency that can be attributed to an ongoing pattern in the community, or what Shaw and McKay referred to as the character of the community. If these residual changes appear to be significantly associated with the changes in the ecological indicators used in the analysis, this may be taken as a sign that the process of invasion and succession in these areas was related to concurrent changes in the levels of delinquency. Finally, since all of the communities are used to estimate the regression equation which predicts the levels at time t , the predicted scores are automatically adjusted for changes that other communities have undergone during the same 10-year period.

The data permit a study of change during three time periods: 1940–50, 1950–60, and 1960–70. Hannan and Young (1977) suggest that pooling these three change periods into one large model with 3×74 community observations leads to a considerable increase in the statistical efficiency of

the model. However, they note that this procedure should only be used if the structure of the relationships remains constant (stationary) over time. As will be shown in the next section, this is definitely not the case with these data. Therefore, a separate regression analysis was computed for each time period, taking the form $D_{\text{res: } t, t-10} = f(P_{\text{res: } t, t-10}, NW_{\text{res: } t, t-10}, FB_{\text{res: } t, t-10}, DN_{\text{res: } t, t-10})$, where $D_{\text{res: } t, t-10}$ is the residual-change score of delinquency during the 10-year period t and $t - 10$, and the other variables are defined similarly.

Although a regression format is used, no causal interpretations should be imposed on the findings for two reasons. All four of the demographic variables are part of what might be called the "ecological complex," that is, they reflect the outcomes of the same ongoing process and therefore are indicators of the same latent construct of invasion and succession. Other areal research employing factor analysis (e.g., Bordua 1958-59; Schmid 1960a) has also recognized the concomitant nature of these relationships. As Bordua notes (p. 238) in a discussion of Lander's (1954) anomie interpretation of a factor structure, "clearly, then, 'anomie' cannot cause delinquency. Delinquency is a species of anomie." In addition, there is no a priori evidence for the theory that changes in delinquency are caused by changes in demographic variables, as opposed to the "selective migration" hypothesis that proposes that there may be differences in the types of people who migrate to high- or low-delinquency areas (Polk and Halferty 1966). Thus, the hypothesis to be tested is simply whether changes in delinquency rates and changes reflecting ecological processes are associated.

Relatedly, even if causal modeling were attempted, it is nearly impossible to separate the effects of the four independent variables from each other due to the high degree of multicollinearity among these indicators of change. Gordon (1968) has also raised this criticism of ecological delinquency research that attempts to separate the effects of highly correlated explanatory variables. The severity of this problem in the 1940-70 Chicago data set is illustrated in table 1, in which each of the demographic variables in a given equation is regressed on the other ecological change variables. For each of the three time periods, the residual changes in the percentage of foreign-born whites, the percentage of nonwhites, and the household-density measure share a great proportion of their variance in common. On the other hand, the residual change in population size is consistently the most independent indicator of this process. Taken as a group, three of the variables, therefore, seem to reflect the same general ecological process. As Johnston notes (1972, p. 160), this degree of multicollinearity results in very imprecise estimates of the individual regression weights because the estimates may have large standard errors, these errors may be correlated, and the sampling variances are very large. Therefore,

TABLE 1
PROPORTIONS OF SHARED VARIANCE IN THE
ECOLOGICAL CHANGE VARIABLES

Residual-Change Variable	R^2 with Other Ecological Change Variables
1940-50:	
Population size.....	.321
Percentage nonwhite.....	.787
Percentage foreign born.....	.695
Household density.....	.610
1950-60:	
Population size.....	.143
Percentage nonwhite.....	.662
Percentage foreign born.....	.630
Household density.....	.565
1960-70:	
Population size.....	.122
Percentage nonwhite.....	.347
Percentage foreign born.....	.474
Household density.....	.289

the individual effects of each of the independent variables will not be discussed except when a variable appears to share little of its variance in common with the other independent variables. Instead, the emphasis will be placed on the overall degree of association between the residual changes in delinquency and the residual changes in the four demographic variables, with the interpretive emphasis placed on the adjusted R^2 statistic.

A reader familiar with the work of Shaw and McKay may immediately wonder why indicators of changes in socioeconomic status during these periods are not incorporated into the analysis, since Shaw and McKay concluded that the economic status of a community was the strongest correlate of the delinquency level of that area. As was noted in the introduction to this paper, the present research focuses on a specific aspect of the Shaw and McKay work—the association of changes in the racial and ethnic composition of an area with delinquency—and does not try to encompass the vast complexity of their research in its entirety. However, it can be validly argued that changes in the socioeconomic status of an area can strongly modify the observed relationship between ecological succession and changes in delinquency. This possibility was investigated, but an inclusion of two additional indicators in the model (median education level and percentage employed as professionals or managers)⁶ does not significantly increase the explanatory power of the model for any of the three

⁶ The most obvious indicator of socioeconomic status, median income (or some related measure), was not available in the 1940 census material. Therefore, these two variables were used as surrogates. Hunter (1974), faced with the same problem, solved it in a similar fashion.

periods. Thus, although changes in socioeconomic status are of great interest, they do not add information over that already contained in the four indicators. Since the interpretive emphasis of the paper is associative rather than causal, there seems to be little danger in excluding them from the model.

FINDINGS

The results of the regression analyses described above, presented in table 2, illustrate the utility of longitudinal data in the statistical analysis of areal structures. The residual changes in the level of delinquency between 1940 and 1950 are only randomly associated with concurrent changes in the indicators of invasion and succession. Thus, in this earliest period, the basic thesis of Shaw and McKay is confirmed—the overall process of ecological change does not seem to be associated with simultaneous changes in the delinquency rates of an area. Although the beta weight for the non-white indicator is significant, recall that this effect is highly confounded with those of the other variables in the analysis. The important measure of the association is the R^2 statistic, which is an unbiased estimate of the total joint relationship between the ecological change variables and changes in delinquency rates.

However, between 1950 and 1960, community change is associated with over one-third of the total variance in the changes in areal delinquency rates. Since the beta weight for residual changes in population size is moderately unconfounded with the other effects in the model (see table 1), table 2 indicates that these changes were negatively associated with residual changes in delinquency rates during this period. Delinquency change is also associated with the joint effects of the foreign-born white, non-white, and household-density indicators. Although the only significant beta weight is for the nonwhite variable, this is misleading, and the three variables must be considered as a parcel since the individual effects are again highly confounded. Therefore, during this period, the Shaw and McKay thesis is no longer supported since changes in these ecological variables are reflected in changing community levels of delinquency.

In the 1960–70 period, the ecological processes are again associated with changes in delinquency rates, although the relationship is not as strong as during the previous 10-year span. In this final period, the household-density item and the racial and ethnic change indicators again emerge as factors strongly related to changes in the levels of delinquency. However, residual population change appears to drop out of the picture during this period.

TABLE 2
REGRESSION OF CHANGES IN COMMUNITY DELINQUENCY LEVELS ON CHANGES IN THE ECOLOGICAL VARIABLES

VARIABLE	1940-50					1950-60					1960-70				
	B	SE	Beta	F	B	SE	Beta	F	B	SE	Beta	F	B	SE	F
DN.....	-.206	.181	-.212	1.292	.393	.288	.191	1.859	.267	.325	.099	.677			
P.....	.000	.000	.102	.522	-.000	.000	-.242	5.888*	.000	.000	.074	.474			
NW.....	.227	.111	.518	4.197*	.258	.082	.477	9.871***	.154	.050	.430	9.510***			
FB.....	.529	.451	.247	1.375	-.216	.415	-.083	.270	-.114	.264	-.054	.185			
Constant.....			.000003				.000006				.000002				
Adjusted R ²012				.379***				.253***				

* Significant at $p < .05$.

** Significant at $p < .01$.

*** Significant at $p < .005$.

DISCUSSION

The evidence presented in this paper indicates that the Shaw and McKay thesis concerning the independence of invasion/succession processes and changes in delinquency rates is supported through 1950. However, since that time, neighborhoods undergoing compositional changes tend to be characterized by changing rates of delinquency. We do not at all want to imply that, in retrospect, Shaw and McKay were wrong. Rather, it is our contention that they worked within a specific historical context and grounded their analysis in a model of ecological processes that have changed dramatically since the publication of the 1942 monograph.

In his introduction to the most recent edition of Park and Burgess's *The City*, Janowitz (1967, p. ix) notes that Burgess developed the ecological model that underlies the work of Shaw and McKay on the assumption of "natural growth patterns." That is, his model was characterized by the unspoken postulate that an ongoing evolutionary process was reflected in the dynamics of spatial distributions. However, it must be recognized that Burgess's natural cycles of invasion and succession were theoretical generalizations of his model, for as Cressey pointed out soon after (1938), the implications of these changes could differ depending on the types of groups involved, that is, "conflict may accompany invasion, varying in intensity with the cultural differences and prejudices of the groups involved." Guest and Weed (1976) have documented in great detail that not all immigrant groups were permitted (or desired) to integrate themselves into new communities. They concluded that, as opposed to the "natural" assimilation process suggested by Burgess, there was clear ethnic segregation between 1930 and 1960 in Cleveland. The assimilation process of one immigrant group into northern urban areas was clearly distinct in Guest and Weed's analysis: blacks were always highly segregated from all other groups. We propose that changes in the invasion/succession processes involved in the black settlement of Chicago since 1950 are integrally connected with the emergence of the invasion/succession and delinquency association since that time.

As Cressey has pointed out (1938, p. 65), blacks were always present in Chicago—in fact, the city was first settled by a black man, Jean-Baptist Pointe du Sable—although, until World War I, they were relatively few in numbers and widely dispersed throughout the city. Even though blacks were a small minority in the early stages of Chicago history, whites were always very antagonistic toward their presence, making integration into the city very difficult (see Branham 1977). Tuttle (1970), for example, uncovered the occurrence of at least 26 bombings aimed at isolated black residences in once all-white neighborhoods from July 1917 until the Chicago race riots of 1919. As Cressey stated (1938, p. 63), "violent oppo-

sition . . . has arisen particularly in the expansion of the Negro population into white communities." Because of the extreme intolerance of the white population, as blacks migrated into Chicago they were forced to settle in the few sections of the city where a nucleus of blacks already resided, called the "black belt." Although this "belt" expanded somewhat by slowly adding new areas contiguous to this residential concentration, the areas of the city occupied by blacks remained generally the same between 1920 and 1950 (Duncan and Duncan 1957, pp. 87-107).

When nationally imposed immigration quotas began to shrink the potential pool of cheap white labor, employers began to redirect their efforts toward attracting blacks from the South.⁷ The effect on the demographic makeup of Chicago was tremendous: between 1940 and 1950, the percentage of blacks in Chicago increased from 8.2% to 13.6%. By 1960, it had grown to 22.9%, and by 1970 it reached 32.7%. Although the black belt was expanding very gradually between 1920 and 1950, it was not doing so at a rate that could easily accommodate the great influx of black immigrants. Coupled with a decrease in housing construction, this immigration process resulted in an extremely dense population concentration in this area; as Abrahamson has stated (1959), the black community was literally "filled to bursting" in the late 1940s. However, in 1948, the United States Supreme Court handed down two landmark decisions (*Shelley v. Kraemer* and *Hurd v. Hodge*) that held that neither state nor federal governments could enforce race-restrictive covenants in housing. With such legal barriers removed, the residents of the black belt began moving into neighboring white areas in unprecedented numbers, and, due to the antipathy of whites toward blacks, the whites moved out. This process was accelerated by real estate speculators who fed on the white population's fear of blacks, decreasing property values, and crime. Many whites immediately sold their property at low prices or abandoned their apartments and fled. Thus, as noted by the National Advisory Commission on Civil Disorders (1968, p. 278), the settlement patterns of blacks became one of "massive racial transition rather than gradual dispersion." It is important to note that, as has been true throughout Chicago's history, whites reacted very strongly to these changes. Hirsch (1977) has documented at least six serious racial disturbances that occurred in Chicago between 1947 and 1957 revolving around the movement of blacks into previously all-white areas.

The trend in Chicago between 1940 and 1970 can be illustrated with an adaptation of Taeuber and Taeuber's (1965) paradigm of invasion and

⁷ Duncan and Duncan (1957, p. 87) estimate that four-fifths of the growth of the black population in Chicago between 1920 and 1940 and seven-tenths of the growth between 1940 and 1950 was due to the excess of in-migrants over out-migrants.

succession. Three types of community changes are defined:⁸ (1) nonwhites increase their composition by at least 10% during the change period but remain in the minority in the community; (2) nonwhites increase their composition by at least 10% during the change period and move from minority to majority status; and (3) nonwhites increase their composition by at least 10% during the change period and are in the majority in the community all 10 years. For easy reference, the first might be called *foothold* change, the second *turnover* change, and the third *entrenchment* change. Within this scheme, 12 communities were classified as racially changing between 1940 and 1950, 14 between 1950 and 1960, and 22 between 1960 and 1970.

Although the number of changing communities during each period is far too small for detailed statistical analysis, their distributions within the three categories is illuminating. As shown in table 3, between 1940 and 1950, 75% of all changes regarding nonwhites were of the foothold variety. However, between 1950 and 1960, well over half of the neighborhood changes reflected radical turnovers of the areas from white to nonwhite. In the final period, the change scores are more dispersed.

The key question, then, is why this change in the dynamics of black invasion/succession was associated with positive changes in the delinquen-

TABLE 3
PERCENT DISTRIBUTIONS OF COMMUNITY
CHANGE TYPES, 1940-70

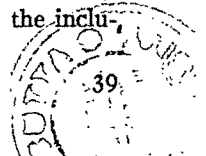
TYPE OF CHANGE	CHANGE PERIOD		
	1940-50 (N = 12)	1950-60 (N = 14)	1960-70 (N = 22)
Foothold.....	75	29	41
Turnover.....	25	64	32
Entrenchment.....	0	7	27
Total.....	100	100	100

⁸ The stress is on the word "change." Therefore, this paper will not discuss areas which remained predominantly (over 90%) white, nonwhite, or stably intermixed during the 10-year periods. The use of the 10% qualification is to avoid discussing changes of a very few percentage points which may simply reflect random and/or measurement errors. However, the 10% cutoff is an arbitrary one. Taeuber and Taeuber (1965) utilized the notion of a "tip point" in the invasion/succession distinction, defined as the "percentage Negro in an area which exceeds the limits of a neighborhood's tolerance for inter-racial living" (p. 100). Since Taeuber and Taeuber utilized the 10% guideline, it has also been adopted here, although this is a highly social-psychological notion and is variable across individuals and communities. The reader should recognize that it is at best a rough operationalization and is used primarily for illustrative purposes. Although the term "nonwhite" appears in the following discussion rather than black, this is not a real change of emphasis, since the overwhelming majority of the nonwhite population of Chicago between 1940 and 1970 was black.

cy rate. A naive and racist interpretation is that it is the presence of blacks per se that is conducive to delinquency. Kapsis (1978), in a study of three primarily black areas of Richmond, California, presents strong evidence that such a conclusion is completely unwarranted. Each of these three areas had undergone various degrees of racial change between 1960 and 1966. Although he does not investigate the related dynamics of change in the delinquency rates, he does find that the delinquency rate in 1966 is "highest among black adolescents living in a neighborhood where the rate of racial invasion/succession is highest and lowest among those living in the racially most stabilized neighborhood" (p. 470). The Chicago data show a similar pattern. Communities defined as turnover had a mean residual delinquency change score of 11.7, compared to 3.87 for foothold communities and 0.12 for entrenchment areas. Thus, neighborhoods undergoing the fastest racial change were characterized, on the average, by about 12 more offenses per 1,000 youths than could be expected from their previous patterns. The most established nonwhite, changing communities (entrenchment areas) had delinquency rates not much different than would have been expected from their previous patterns.

Such findings suggest that it is the nature of change that is related to delinquency rather than the groups involved. The preceding paragraphs have documented that patterns of community change for blacks have altered dramatically, at least temporarily, in Chicago's recent history. In his addition to the 1969 edition of *Juvenile Delinquency and Urban Areas*, McKay alters the statement that he made with Shaw concerning invasion and succession to note that in communities where there have been steady upward or downward trends in delinquency, the "areas showing the greatest increase have experienced disruptive changes brought about by new migration, while areas of greatest decrease are previously high areas in which institutions have stabilized during the period of study" (p. 345).

It is our position that the word "stability" is the key to the interpretation of the association of delinquency with these processes since 1950, although, unfortunately, data concerning institutional stabilization are sorely lacking and the argument must remain grounded in conjecture. Cressey (1938, p. 63) has implied that perhaps the most difficult part of the invasion/succession process is the reorganization of the social life of the invading group as it acquires dominance in an area. This posed an especially difficult problem for the blacks involved in this rapid transition. As Berry and Kasarda have argued (1977, p. 58), the local community is "... an ongoing system of social networks into which new generations and new residents are assimilated." When the existing community changes almost completely within a very short period of time, the social institutions and social networks may disappear altogether, or existing institutions may persevere in the changed neighborhood but be very resistant to the inclu-



sion of new residents (for a detailed example of this phenomenon in Chicago during the 1950s, see Abrahamson [1959]). In addition, since so many of the new black residents were recent immigrants from the South, they may not have had time to fully develop the kinds of institutions that could be imported from one community to another, for as Berry and Kasarda found (1977, pp. 59-71), participation in local social networks is primarily a function of length of residence in the community.

Thus, it appears that, during the time it takes to stabilize and establish a community, delinquency is more likely. This seems to be a logical explanation of why the association of change with delinquency is highest between 1950 and 1960 (when the most rapid changes were occurring) and why it declines somewhat between 1960 and 1970, when these communities have had at least a short period of time to reestablish themselves.

CONCLUSIONS

Janowitz (1967, p. ix) has called for a revision of Burgess's model of human ecology because Burgess could not foresee the rapid social changes of recent years. Likewise, Shaw and McKay should not be faulted for imperfectly seeing the course of the future. However, those readers familiar with the Shaw and McKay tradition will immediately recognize that notions concerning stabilization were central to other parts of their ecological model of delinquency. One of the underlying themes found in their interpretation of the finding that delinquency rates decreased with distance from the center of the city is that as a group progresses upward in terms of social mobility, it moves into progressively higher-status areas which were assumed to be more stable. In fact, the Chicago Area Project, a delinquency program founded by Shaw in the late 1930s (see Kobrin 1959; Finestone 1976), was explicitly formulated around the stabilization issue.

The association between changes in delinquency rates and neighborhood composition was not apparent during the periods that they analyzed because, generally, there were traditional, nonstable areas close to the center of the city that served as conduits for incoming white immigrant groups. It is important to emphasize that these areas were characterized by high rates of delinquency, no matter which groups were moving into the area, just as the new communities serving as conduits for blacks since 1948 were. Since 1948, entirely new neighborhoods began to serve as the conduits for settlement due to the historical factors noted above, shifting these areas temporarily from stable to nonstable. This was a pattern of community change that Shaw and McKay could not foresee. However, although the pattern has been altered, their formulations concerning the relationship between processes of community stabilization and delinquency rates still appear to be amazingly robust.

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Knowledge, Meaning, and Social Inequality in Kenneth Burke¹

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In spite of his influence on American letters, Kenneth Burke has little influence on sociology. Those few sociologists making use of his work rely on his theories of dramatism and symbolism. This paper presents an alternative, complementary analysis of Burke. First, it shows he does not distinguish knowledge and meaning as some who use his work do. Next, it shows that although Burke emphasizes interpretations as a cause of action, he sees interpretative frames as social, external, and constraining, derived from the dominant mode of production. They help legitimate and reproduce the social order. However, because of man's material aspect, reproduction is imperfect.

Kenneth Burke has been an important figure in American thought and literary criticism since the publication of his first book, *The White Oxen and Other Stories*, in 1924 (see, generally, Rueckert 1969). He was an important figure in the New Criticism and has written a great deal on language, human action, and society. In spite of his massive and far-reaching production, sociologists have not made much use of his ideas. Part of the reason for this is his marginal position in American academic life, and it is worth spending some time on his intellectual and professional milieu.

His first essays appeared in the early 1920s,² and by the late 1920s he was publishing regularly in the *Dial* and the *New Republic*. In the late 1930s he began to shift publication to the *Kenyon Review* and the *Sewanee Review*. This allegiance lasted until the 1960s, during which he did not publish regularly in any particular journal. This publication history is significant because it shows he did not publish consistently or extensively in academic, as distinct from literary, journals, and perhaps partly because of this there has been little treatment of Burke in scholarly journals.

This mutual nonrecognition applies to Burke and academic sociology as well. He published only two journal articles in sociology, in 1939 and

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² Unless noted otherwise, I base my comments here on publications by Burke on Frank and Frank (1969), and my comments on publications about Burke on Rueckert (1969, pp. 515-22). Also, throughout the paper, except where noted I have neither added nor deleted emphasis in quotations.

1942, both in the *American Journal of Sociology*. In return he received only book reviews, of *Permanence and Change*, *A Grammar of Motives*, and *A Rhetoric of Motives*, until the later 1950s when H. D. Duncan, followed by a very few others, began to use his thought more extensively.

Nonrecognition was more striking in Europe. Prior to the Second World War there appeared only one treatment of him, in the English literary review the *Calendar*, in 1926. After the war things were no better. In 1948 Bewley published a scathing review of Burke in *Scrutiny*, and the 1954 *Times Literary Supplement* special edition on American writing was equally condemning (see Frank 1969). One can explain the English response in terms of the dominance of F. R. Leavis's strongly textual school of criticism, which did not mesh with Burke's tendency to seek insight wherever he might find it.

As A. P. Frank notes in his discussion (1969, p. 440) of Burke's reception in England and Europe, he "maneuvered himself into a strategically disadvantaged position in relation to the mainstream of modern critical thought." This lack of positive response by literary circles is echoed in the general ignorance of Burke among academic sociologists.⁸

What must be an important reason for Burke's marginal position in American academic life is his isolation. He taught sporadically at the New School in the 1930s, at Chicago in 1938, and then at Bennington College during alternate weeks in the 1940s and 1950s (see Fogarty [1959] 1969, pp. 324-25; Kostelanetz 1981). During all that time he lived in an isolated part of New Jersey and kept himself pretty much to himself and a small circle of friends, as he continues to do. In other words, he never has been at a university long enough to develop a group of postgraduate students who would spread his ideas. The other side of this is that he has isolated himself from academic currents and concerns. He compounded his isolation with a very long gap between the publication of *A Rhetoric of Motives* in 1950 and *The Rhetoric of Religion* in 1961. Rueckert (1969, p. 297) notes that the latter book "was hardly reviewed at all and only his most devoted followers even heard about it. . . . Burke . . . has not yet really recovered from his ten years of silence."

BURKE IN SOCIOLOGY

There are some important exceptions to the general sociological neglect of Burke. For instance, the work by Dell Hymes (e.g., 1964) on the social use of language reflects the fact that he studied under Burke. Occasional reference to Burke appears in symbolic interactionist writings (e.g., Manis and

⁸ The only real exception I know of is the French anthropologist D. DeCoppet at the Centre National de la Recherche Scientifique in Paris, who has run a sporadic Burke seminar.

Meltzer 1972), particularly making use of his dramatistic method. This seems to be the most widely used aspect of Burke's writings, and recently Overington (1977a) published an extended sympathetic analysis of it. Also, Overington (1977b) has published a somewhat less extensive and necessarily much more synoptic review of Burke's substantive concerns. Like many users of Burke's writings, he concentrates on the analysis of motive and especially the language of motive as revealed in accounts people produce of events. Thus Overington's scope is rather different from mine in this paper.

Perhaps the most important users of Burke's ideas are Hugh Dalziel Duncan, Clifford Geertz, Erving Goffman, and Joseph Gusfield. All have borrowed more or less extensively from his writings, using his ideas to help fashion, illuminate, and explain their own concerns. Because they are the dominant sociological users of Burke, inevitably they tend to define Burke for sociologists. With the partial exception of Duncan, however, they have not attempted a systematic exposition of what the man has to say. Rather, they have taken from him what seemed most useful and interesting with respect to their own concerns. As a consequence they all, including Duncan, focus mostly on what Overington (1977b, p. 137) identifies as Burke's later period, marked by his extensive development of dramatism and a more purely linguistic and symbolic model of human existence.

In doing so they have unintentionally diverted our attention from the important social structural foundation Burke lays down to support his model of symbol use. I do not mean this point to be taken as criticism of these authors. As I said, they were using Burke, not presenting him. However, the use they have made of him, coupled with the fact that they have defined Burke for sociologists, has led to what I consider an excessively idealistic and voluntaristic view of the man's work among academic social scientists.

I shall show this in this paper. First, I discuss very briefly how these authors approach symbols in society. I do not intend to produce a systematic analysis of Duncan, Geertz, Goffman, or Gusfield, nor is it necessary to do so. Instead, I shall look at their general approach to the sociology of knowledge and symbols. Then I turn to a discussion of Burke. As will become clear, his concern for power and domination in society and the way these affect knowledge is not reflected in these authors.

Duncan uses Burke the most systematically, producing a theory of the social order as "a drama of social hierarchy in which we *enact* roles . . . through communication" (1962, pp. 10-11). His interpretation of Burke seems rooted in the voluntarism of the Parsonian functionalism which was prevalent when he was writing *Communication and the Social Order*. This and the elements of Burke he chooses to stress make his use of Burke partial in two respects.

First, his concern with consensus achieved by actors lends too much of an air of harmony and volition to his discussion. Intentionally or not his concern with consensus and the image of the actor both suggest agreement arrived at freely. Elements of Burke's writings concerned with power and social determination do not fit the general tone of Duncan's work. Second, he illustrates much of what he has to say about the communication of symbols and meaning with discussion of pageants, art, and other overtly symbolic statements. This choice is unfortunate because these are the things we identify as peculiarly theatrical and symbolic; they are divorced in important ways from one of the things Burke is concerned with, the mundane acts of everyday life.

Goffman relies heavily on Burke's dramatism (see Mitchell 1978, pp. 84-91), especially for his view of social behavior as performance, impression management, albeit one in which the audience collaborates to some degree. As such this approach shares the incomplete quality of Duncan's rendering; especially the implication, stronger here than in Duncan, that people are able to step out of their roles more or less at will, even if only to step into others. Thus Goffman does not make use of Burke's concern for externality and constraint.

Related to this, Goffman's interest lies in the relatively microsocial, with particular scenes, settings, and interactions. This means he tends to ignore the larger social processes which Burke sees as crucial to an understanding of social relations. In *Frame Analysis* (1974) Goffman tries to correct this by giving more weight to the setting as a constraining force, but even here the setting is defined in terms of, and thus in some sense subordinated to, the acts it contains.

Geertz is more interested in problems of social and political organization and structure than either Duncan or Goffman. However, he deals with these problems not as Burke does, in terms ultimately of social and economic structure and practice, but in terms of culture, in the laudably limited and hence useful sense he puts on that word. He says (1975, p. 5) his task is "construing social expressions on their face enigmatical," in order to explicate the "webs of significance [man] himself has spun." A very necessary task, for as he notes, it is on these self-spun webs that men stand. So, for example, his discussion (1975, pp. 255-310) of state formation in "The Integrative Revolution" deals with cultural unity and the symbols of nationhood rather than social and economic forces and the noncultural mechanics of power. In saying that "ideas . . . must . . . be carried by powerful social groups to have powerful social effects" (1975, p. 314), he recognizes the significance of power. But like Duncan and Goffman he prefers to limit himself to questions of identity, impression, meaning, and symbols.

Gusfield's work, especially his *Symbolic Crusade: Status Politics and the American Temperance Movement*, comes closest to what I consider a

rounded interpretation of Burke. His concern in that book is to show how changes in the temperance movement reflected the changing position of different groups in the American social structure. In his concluding chapter he makes explicit his heavily Burkean dramatic model of status politics, stating at the outset (1963, p. 166) that "political action has a meaning inherent in what it *signifies about the structure of society* as well as in what such action actually achieves" (emphasis added). Thus does Gusfield, like Geertz, pay particular attention to the links between the symbolic and the political and social.

His concluding discussion revolves around prestige, esteem, status, deference, and styles of life; it is concerned with the symbolic side of the social order. Gusfield justifies his approach by saying (1963, p. 180) that these things are important because "men's regard for respect, honor, and prestige is real." In this he does not go as far as Burke, who, as I shall show, thinks they are important not only because men's regard for them is real but also because they reflect, shape, and indeed are part of power in society.

None of these writers represents Burke's ideas as I think proper—not just as a social philosophy of the communication of symbols and meaning among interacting individuals and groups, but also as a sociological model of knowledge, power, and social structure. In saying this I am not denying the utility of their uses of him. Rather, I mean to point out that none of them uses him as a way of dealing with what seems to me one of the crucial problems he addresses: how the social bases of knowledge help legitimate and maintain inequality in ways which are neither obvious nor obviously coercive. This is what Steven Lukes (1974) quite properly identifies as a central aspect of the problem of power in society, and what Michel Foucault has investigated extensively.

I want to make one final comment in this discussion of Burke in sociology. It is worth noting that the phenomenon of those who refer to Burke but do not provide what I consider an adequate representation of his sociology of knowledge is paralleled by the phenomenon of those who present a remarkably Burkean view of knowledge and society but do not refer to him.

This is notable particularly in the case of Pierre Bourdieu, influenced most heavily by Durkheim, Marx, Lévi-Strauss, and phenomenology. In his *Outline of a Theory of Practice* (1977) Bourdieu, like Burke, shows how symbol and fact blend into a coherent whole, the habitus, which becomes embodied in social practices and artifacts and which reflects, supports, and helps reproduce the social hierarchy. Also like Burke he notes in this book how what he calls the cultural arbitrary—loosely the values and beliefs which justify the way the society is organized—is legitimated by naturalization, by seeing it as a reflection of the natural order of

things rather than as a product of social relationships of dominance and subordination.

Bourdieu carries this linkage between knowledge and society one step further in *Reproduction in Education, Society and Culture* (Bourdieu and Passeron 1977), a discussion of how reproduction of symbolic structures across generations is instrumental in the reproduction of social structures. This approach appears in Basil Bernstein's work on education (esp. 1975). However, while Bourdieu has influenced Bernstein, there is no evidence that Burke has influenced Bourdieu.

THE UNITY OF KNOWLEDGE AND MEANING

I said that the writers discussed above have used Burke to provide a basis for their understanding of symbols in society. In this section I want to show that Burke provides not only a theory of symbols but, more fundamentally, a sociology of knowledge. This distinction is important, because "symbol" carries a connotation of fluidity, negotiability, and subjectivity, while "knowledge" does not. As I shall show, Burke does not think that people can produce objective knowledge without symbolic meaning. For Burke symbols are not added on to a hard objective reality or hard objective knowledge; instead, these merge inevitably in the human mind, so that using them in a dichotomy is improper.

Much of *Permanence and Change*, an antiscientistic and antisubstantivist book very much in the mold of the writings of the Frankfurt school,⁴ revolves around Burke's theory of knowledge, and although it is a relatively early book I see no reason to assume he abandons the theory later. His basic point here is that objective, substantive neutral knowledge does not exist, however strongly we may think it does. And without this sort of knowledge we cannot distinguish the natural and factual from the social and meaningful or symbolic. He does not reject the existence of the objective world, he just denies that we can have unmediated access to it.

His basic position is that from the human point of view, the only one we have, "reality is *what things will do to us or for us*. It is expectations of comfort or discomfort, prosperity or risk" (Burke [1935] 1965, p. 22). This linking of what can be called fact and value is based on the dialectic of tragedy, Burke's dialectical view of knowledge. He says that knowledge is acquired when an individual does something purposeful. This "brings to the fore whatever factors resist or modify the act. . . the agent [i.e., the actor] thus 'suffers' this opposition, and as he learns to take the oppositional motives into account . . . he has arrived at a higher order of understanding" ([1947] 1969a, pp. 39-40). In other words, we acquire new knowledge ("a

⁴ Hamilton (1974, pp. 55-65) presents a good brief discussion of the Frankfurt school.

higher order of understanding") when things either help us in or hinder us from achieving the goal of our action. Thus, our knowledge of things springs from practical action, itself a result of the interaction of motive on the one hand and objective conditions on the other.

Because knowledge springs from practical action, it is conditioned by and thus reflects the purposes underlying that action. Truly objective knowledge, devoid of meaning and symbolism, could derive only from purposeless activity. Burke thinks the possibility of such activity is absurd. Therefore, humans cannot produce objective knowledge. All that we know is tinged with meaning, carries a symbolic load. For Burke, our knowledge of "things and operations smuggle[s] in connotations of *good* and *bad*, a noun tending to carry with it a kind of invisible adjective, and a verb an invisible adverb" ([1935] 1965, pp. 191-92).

Burke does not limit practical action to the manipulation of physical objects. Rhetoric—communication intended to persuade others and so induce cooperation and lay the basis for collective action and social order—also is a practical activity. One presents ideas to persuade one's fellows, and their interpretation, expressed in criticism or suggestion, acceptance or rejection, provides the interaction leading to the dialectical growth of knowledge, knowledge about both one's fellows and one's ideas.

Although he sees knowledge as the result of this dialectic, a consequence in part of meaning and purpose, and through them social relations (as I shall show below), Burke says that people themselves do not see knowledge this way. He says (1952, pp. 259-60) that one goal of his dramatistic method is to overcome this naive substantivist view of knowledge, "to make us sensitive to the 'ideas' lurking in 'things,' which might even as social motives seem reducible to their sheerly material nature, unless we can perfect techniques for disclosing their 'enigmatic' or 'emblematic' dimension."

People do not think, then, that knowledge springs from a dialectic but from a straightforward awareness of the objective, substantive nature of the object of knowledge, the thing or process known. Knowledge naively apprehended is a reification of the consequences of dialectical interaction and thus is alienation, in Marx's sense of people reifying and becoming alienated from the results of their own practical activities (e.g., the Theses on Feuerbach). However, Burke does not follow Marx in basing this alienation on class or capitalism. Instead he takes a self-consciously Kantian view that this reification "is a form of the mind (or, in my narrow usage, an inevitable aspect of linguistic placement)"⁵ (quoted in Holland 1959, p. 36).

For Burke, then, knowledge is necessarily infused with meaning, factual

⁵ Marx's comment that relations of production "in language . . . can only be expressed as concepts" (quoted by Bottomore and Rubel 1964, p. 78) suggests he was aware of the substantive nature of language and thus of the difficulties it presents to those trying to grasp dialectical processes and products.

reality is necessarily infused with symbol, so that the distinction between them is false. However, because of the nature of language people lose sight of this and see facts substantively, nondialectically, as more or less neutral reflections of objective reality. This lays the groundwork for a false distinction between knowledge and meaning, reality and symbol.

SOCIAL BASES OF KNOWLEDGE

As many of the people who use Burke are quick to point out, he stresses that people react not to objective reality but to their interpretations of it, as is implicit in the preceding section. He says, "Any given situation derives its character from the entire framework of interpretations by which we judge it" ([1935] 1965, p. 35). This sets Burke apart from a naive materialist reductionism but does not provide warrant for the pair of conclusions some users of Burke seem to have reached, even if only implicitly; first, that the *existence* of interpretation implies *freedom* of interpretation, if only we have the will to grasp that freedom; and second, that the bases of interpretation constitute or are part of some idealistic realm of the symbolic, relatively unconditioned by material forces.⁶

On the other hand, as I shall show in this section, Burke presents a rather deterministic view of interpretative frameworks, full of Durkheimian externality and constraint and grounded in a Marxian way on practical activities conditioned by social and economic relations. In saying this, however, I do not mean to imply that he makes the error of the structural-functional extremists, seeing the social "as *constitutive*, rather than regulative, of the self," as Merquior (1979, p. 55) puts it. Burke defines man as the symbol-using animal (1966, p. 16), and he is quite aware that the social, symbolic side is only part of the whole. The individual, non-symbolic aspect, what he calls ([1941] 1973, p. xvi) the realm of motion, as opposed to action, provides a counterweight. I shall discuss this after I lay out Burke's view of the social and symbolic.

Burke says ([1935] 1965, p. 15) that one important basis of interpretation is orientation, "a bundle of judgments as to how things were, how they are, and how they may be." The orientation is the sum of all we know; the meaningful construction of the world embodied in our knowledge of it. In much of his literary criticism (e.g., his extended treatment

⁶ These appear, e.g., in Goffman (1967, p. 5), with his discussion of how an actor "expresses his view," or "claims for himself" a certain social value, and more generally in his consideration of the rules of behavior, which he renders implicitly manipulable by failing to identify them in terms of the constraints imposed on individuals by the larger social order. More curiously these conclusions appear as well in Overington's (1977a, p. 139). Idealistic suggestion that Burke's "perspective by incongruity," a form of individual mentalistic symbolic manipulation, "is not simply an instrument for interpreting the social world, it also gives the possibility of *changing* the world!"

of Coleridge in the title essay of *The Philosophy of Literary Form*), he pays great attention to the idiosyncrasies of an individual author's orientation, which makes it easy to forget that he sees orientation as an overwhelmingly social product.

Thus, for Burke such idiosyncrasies as exist are variants on a theme of *collective* representations, themselves embodied in the language of the group, "a social product" (1966, p. 361). An awareness of the social, and indeed the material, basis of orientation and the structures of symbol and meaning it contains is fundamental. Language and all it embodies is "a device which takes shape by the cooperative patterns of the group which uses it" ([1935] 1965, p. 175). In other words it springs from "*group* relations. However much the individual . . . may transform language for his special purposes, the resources with which he begins are . . . *social*. And such sociality of meaning is grounded in the sociality of material conduct" ([1935] 1965, p. liii).

For the individual, then, orientation is both external and constraining, though as I shall indicate below it is not perfectly so. It is external by virtue of its social basis, as we are confronted with the "linguistic texture into which we are born" ([1935] 1965, p. 36) and of the group to which we belong, drummed into us by "trivial repetition and dull daily reenforcement" operating socially, "without conscious direction by any particular agent" ([1950] 1969*b*, pp. 26, 35). We are here at the mundane, automatic level of much of daily life, a long way from the self-conscious theatricality of Duncan's pageants and works of art, the self-conscious politicking of Gusfield's teetotaler and Geertz's nation builder, and Goffman's actors concerned with face.

Orientation becomes constraining as soon as we try to communicate, try to persuade others to cooperate with us in collective actions; that is to say, as soon as we try to enter into social relations to achieve our desires. Our fellows are unlikely to respond with cooperation if we do not address them in terms which they understand, asking them to do things they think reasonable.

In order to gain their cooperation the individual must socialize his desires: bring them into conformity with the collective orientation. Burke illustrates the extent and basis of this when he relates Piaget's work on how children develop logic. Burke says ([1935] 1965, pp. 201-2) this happens when children discover that "pure threat or pleading [is] . . . inadequate, and the socializing ways of reason and proof take their place." As I mentioned previously, this is one way we produce knowledge dialectically. In socializing the expression of our desires we learn more about them and so change them. The conformity which may at first be superficial becomes more deep seated, so that we end up desiring what our group recognizes as proper for us to desire.

By invoking Piaget's work on the development of logic, Burke broadens the ambit of the collective orientation beyond knowledge of the world. He subsumes under orientation formal elements of discourse, in addition to matters of substance, and directs us to the same conclusion C. Wright Mills reached (1939) (a conclusion reflecting Burke's influence on him) that logic and other formal elements persuade not because of some inherent objective structure called "logic" with which people resonate but because the group recognizes them as "good argument." In other words, logic has power because socially we learn to treat it as potent and to laugh at those who believe six impossible things before breakfast.

So, for Burke one is socialized into the orientation of the group as a result of the eminently practical activity of trying to get what one wants, just as one learns about the rest of the world. The person acts in order to achieve some desire. Under the tragic dialectic, the basis of knowledge, our acting entails an interaction between the goal which motivates that act on the one hand and, on the other, the external constraints which help or hinder our achieving that goal. Out of this interaction comes new knowledge, of our goals, our means, and the external constraints. In the material world our actions are physical, while in the social realm our actions may also be verbal—a form of symbolic action.

As a result of actions in both the material and social realms we learn about our goals, techniques, and environs, because these three are part of the practical activity we undertake. Thus, in acting in society we learn not only about our fellows. We learn also what are true (i.e., valid) communicative and persuasive techniques; we learn the proper forms of discourse; and we learn what are proper goals and desires. In other words, we learn to become members of the group, to see the world and ourselves and our fellows in terms of the orientation of the group.

Our actions in the physical realm have the same effect. As we learn the group's orientation, it affects our motives as we act in the material realm. That is, we learn to approach the natural realm in socially proper ways; and so the new knowledge that arises from our interaction with the natural realm reflects the group's orientation. Thus, as we learn to approach the natural realm in socially proper ways, so our knowledge of that realm is conditioned by the group's orientation, and thus its practices and structure. As I shall show below, this has important consequences for the legitimation and reproduction of the social order.

I said that orientation springs from practical activities. These are guided in part by a person's interests and occupations. Burke extends occupation far beyond the individual's place in the productive process: "Is it not also an occupation to be a hunchback, or to have read the Bible oftener than the Pope, or to experience a 'run of bad luck'?" ([1935] 1965, p. 237). So, for example, much of Burke's discussion of Coleridge relies on seeing

opium addiction as his occupation, shaping and reflecting his interests and informing his orientation.

But again we have to remember the difference between the idiosyncrasies of the individual authors who are the objects of Burke's literary analyses and the commonalities of the group. Burke is quite aware that the orientation of the group revolves around the fundamental task of producing basic subsistence goods, "certainly primary as regards problems of existence" ([1935] 1965, p. 38). This sort of production is the basic occupation of the group, common to its members, and from this and the social relationship surrounding it come "specific patterns of thought" ([1935] 1965, p. 38), which underlie and dominate the collective orientation.

In grounding the orientation of the group on the processes and social relations of production—the mode of production even though he does not call it that—Burke is not reducing the orientation to them. Here too there is a dialectic at work, and this forms the basis of his criticism of pragmatic materialism and liberalism, both of which he sees as rooting value and meaning purely in practical activities and material conditions, a viewpoint which ignores the dialectic between the technical and material on the one hand and, on the other, the orientation and social relations in terms of which people undertake and learn from these practical activities. Thus he criticizes the pragmatists and liberals for seeing principles of judgment as coming "from the nature of *things*, not from authorities and their precepts" ([1950] 1969*b*, p. 31).

In ignoring the importance of orientation, liberals and pragmatists ignore, and so hide, the importance of interpretative frames and with them the social relationships of authority and subordination which support and reflect them. Thus Burke sees orientations as rooted in the practical production of the means of subsistence, the knowledge and social relationships which go into that production, and the social "authorities and their precepts," the realm of power and politics. In Marxian terminology, he sees the superstructure as being in dialectical interaction with the infrastructure, so that any analysis of one without the other is partial, treating as self-contained and determinate what in fact is part of a larger whole.

KNOWLEDGE, POWER, AND REPRODUCTION

The importance for Burke of power and politics is highlighted in his choice of the constitution as the quintessential form of human symbolic relations ([1947] 1969*a*, pp. 323 ff.). After all, a constitution is a supremely political act of domination, designed and imposed to reinforce a state of social relations which is under some sort of threat, either to destroy it or to prevent it from coming into being. And if it is the constitution of a govern-

ment it is maintained in part by force. Here Burke is pointing to the central place of politics, and hence power, in the symbolic establishment and maintenance of orientation, and with it social relations.

All constitutions seek "to establish a motivational fixity of some sort, in opposition to something that is thought liable to endanger this fixity" ([1947] 1969a, p. 357). In other words, they lay down a definitive way of looking at the world, a definitive body of knowledge, and thus a definitive guide to interpretation and action: in short, a definitive orientation or part of one. They do so by stating and giving primacy to certain facts (self-evident truths) which so define the situation that certain acts and social relations become prescribed and others become proscribed. This is what Burke means by "motivational fixity."

For instance, in the constitutional statement that a right is inalienable, we have in effect the statement that it is "a right 'that *cannot* be alienated' (since the 'nature of things' would make this 'impossible')," in other words, a "statement about 'inevitable' structure" ([1947] 1969a, pp. 361-62). This serves to legitimate the orientation, the guides to action and knowledge it contains, and the social relations it entails, by grounding them on the natural aspect of things; which, remember, Burke says people see substantially, as an objective facticity residing in the object of knowledge, even though their knowledge of it develops dialectically.

This reference to the natural order of things makes a constitution more persuasive, by grounding it in what the people take to be objective reality, the nonsocial realm which exists independent of human existence. But as Burke notes, this grounding is possible only because, following the dialectical basis of knowledge, people understand the nonsocial realm in terms of existing social and political ideas and relations. Thus, "social relations were first *ascribed* to nature, and then 'derived' from it" ([1935] 1965, p. 278n).

The framework provided in a constitution carries with it the authority of the group. As such it authorizes one way of looking at things and unauthorizes others: "By encouraging men to evaluate their public acts in the chosen terms, it serves in varying degrees to keep them from evaluating such acts in other terms" ([1947] 1969a, p. 368). The authority of the group encourages a certain perspective, and the daily acts and interpretations reflecting that perspective reinforce it, so that it becomes taken for granted.

The consequence of this is that the perspective, the orientation embodied in a constitution, becomes both morally and objectively correct in people's eyes, which in turn makes the group and its structures, values, and practices seem the obvious and necessary consequence of objective truth. At the same time, the alternative perspectives lose political and empirical legitimacy; they sink from view, they become nonsensical, they fly

in the face of the will of the group and the empirical world group members see.

Burke's view of constitutions is, then, very much like Marx's view of the English political economists presented in his discussion of the fetishism of commodities in *Capital*. Remember that Marx said their theories took commodity production and exchange as a natural truth legitimating relations of dominance and subordination and delegitimizing alternative social arrangements and courses of action. Further, Marx saw that the economists perceived production and exchange through a fog of unrecognized social values, ultimately based on the class structure, which led them to misrecognize a historically contingent set of social relationships as the expression of a set of nonsocial, transcendent natural laws.

Usually constitutions justify the existing or desired social order by reference to a set of self-evident organizing principles, ideally grounded in nature. Because of such justification, social inequality can gain the legitimacy of a grounding in what people see as natural inequalities. Those at the top of the hierarchy are seen to have personal attributes which best express or embody in the highest sense the natural principles on which the hierarchy rests. This view justifies their dominant positions by making them appear the best qualified to dominate. But it does two other things as well.

First, it provides a bond between the dominant and subordinate groups, as both are portrayed as participating in the hierarchical order grounded on these natural principles. This is what Burke, borrowing from Empson ([1935] 1979), describes as the pastoral element, consisting of devices for transcending hierarchical differences and defusing class conflict by an inversion and celebration of the hierarchical principles underlying the social order.

Burke says the pastoral involves a reversal of values, whereby the subordinate are celebrated for the fitness with which they fulfill their subordinate positions. "Hence we get the long literature of transvaluations whereby humble rustics, criminals, children and fools are shown to contain the true ingredients of greatness" ([1941] 1973, p. 422). When the pastoral moves out of literature and into the group's orientation, it provides a nobility for the subordinate by showing that in their subordination to the hierarchical principles they become equal to the powerful in society, who are powerful only because they too subordinate themselves to those principles. Thus the pastoral provides a kind of unity and ironic equality across social classes, while confirming the subordinate in their subordination and the dominant in their domination.

Second, organizing principles often are used to present to members of the society the view that all, even subordinate members, would be worse off if a different ordering principle, or none at all, were used. Usually this is done by pointing out that the nonarbitrary, proper needs of society are

served best if those who best reflect or embody the ordering principles of the hierarchy (be they ones of grace, taste, or intelligence) are given positions of power. Thereby social inequality becomes justified as reflecting nonarbitrary differences between people as these mesh with the nonarbitrary needs of society as a whole. (A classic statement of this sort of legitimation of inequality is in Davis and Moore's [1945] grounding of it on functional necessity.)

Of course, all this leads to stability, the reproduction of the orientation and the social hierarchy over time. Basically reproduction occurs because people, seeing the world in terms of their orientation and the principles underlying the social order, will try to make their society and their lives more perfect expressions of that orientation and its principles. And this effort will be guided by the dominant groups, best expressing what underlies the social order.

Burke sees this sort of perfectionism as basic to human nature. One need not, however, concur with Burke in seeing it as a necessary consequence of the human use of language. Instead, one can derive it from more mundane processes. Orientation and its underlying principles provide the standard by means of which things and events are evaluated. The best, truest, or most efficient thing or act will be that which in its context conforms best to the dictates of the orientation. Things violating them will be corrected and brought into conformity, and things newly created will reflect and embody the orientation of their makers.

So, the restructuring of the environs helps further to objectify and naturalize the orientation and its associated social and economic order. People through their practical activities transform their natural environs and their natural state into what Burke (1966, p. 15) calls "second nature": "the complex network of material operations and properties . . . that arise through men's ways of livelihood, with the different classes of society that arise through the division of labor and the varying relationships to the property structure" (emphasis omitted). So, to continue a point made earlier, people not only interpret the natural world in terms of their orientation, they also modify that world so that it becomes a concrete manifestation of their orientation and thus of the social order. This process serves further to naturalize the orientation and make its truth more self-evident.

In addition there is reproduction across generations, though Burke does not discuss this explicitly. As children mature in society they will learn the dominant orientation. Further, they will be allocated to positions in the hierarchy according to how well they express the principles underlying it. This too helps make these principles more self-evident, as members of the dominant group will be those best expressing them. Thus, when the next generation matures its members will have recreated the relations of

dominance and subordination which make up the social structure and the orientation which reflects, justifies, and underlies it.

LIMITATIONS ON REPRODUCTION: MAN'S MATERIAL ASPECT

What I have sketched out so far is a model of knowledge and society which is perfectly conservative; an orientation and associated social structure ordering and reproducing themselves with neither conflict nor change. This conservative bias follows from my exclusive attention to the social aspects of human existence, especially the ways the group acts to assure that all its members share the common orientation. As I have noted, much of this conservative reproduction occurs through and because of the use of symbols and communication. Burke, however, is quite aware of the importance of the nonsocial, the nonsymbolic, the material aspect of human existence. He sees it as an important source of division, disagreement, and change, counterbalancing the conservative tendencies of the social and symbolic (the influence of Marx is obvious).

Although the social and symbolic aspects of human existence help lead to cooperation and order, according to Burke humans also exist in this nonsymbolic material realm. Burke defines man as the symbol-using animal, and though much of his work is an exploration of symbol using, he cautions (1955, p. 260): "We must watch lest, in our zeal to bring out the *formal* considerations of the differentia (language-using or symbol-using), we slight the *material* considerations of the genus (animal)."⁷

Divisions and differences between people are impediments to social order and continuity; they are also the hallmarks of the material realm. Most basically they spring from "the human body, with the divisive centrality of its particular nervous system," resulting in a "generic divisiveness which, being common to all men, is a universal fact about them, prior to any divisiveness caused by social classes" ([1950] 1969b, p. 146). By this Burke means that humans are physiologically separate from each other and thus cannot share fully each other's experience, each other's knowledge, and thus each other's orientation.

So even though Burke is quite aware of the significance of social divisions caused by social forces, most notably the division based on class differences, he sees a radical presocial source of division in the physical separation of human beings, a division no symbolic unification can overcome completely. Although the consequences of this material side of human existence appear in many forms, here I shall discuss only the way

⁷ Overington (1977b, p. 137) says Burke thinks that "humanness is . . . *essentially* a product of language." Rather, Burke's statement shows he thinks animality is as essential to humanity, as use of language. As one might expect, Burke sees the two in dialectical relationship.

in which it encourages social separation: the formation of local and potentially competing orientations in local and potentially competing social groups and classes.

Physiological separation leads to the disunity of the social order: it encourages the development of group and class orientations. It does so because people cannot share fully in the experiences of the whole group, so that the unifying power of group symbols and the group orientation is attenuated, with the consequence that local symbols and orientations develop. These occur where the orientations of people are sufficiently alike because their lives are sufficiently alike, that generic divisiveness is overcome, or at least counterbalanced.

I noted earlier that Burke says the mode of production is primary in shaping orientation. However, we must remember that local interests and values, made inevitable by the divisive consequences of man's material aspect, also affect orientation. As a consequence division can occur even among those who occupy the same position in the productive process. Differences in religion, family life, geographic location, and a host of other factors not directly part of production, however much they may be affected by it, can be the basis of division, and hence of subgroup formation. Thus, although the productive process is primary, it is by no means the only basis of such formation. (Parkin [1979] presents an excellent treatment of subgroup formation from a neo-Weberian perspective, fitting nicely with many of Burke's ideas.)

So man's animality is a source of division prior even to social classes. Put most simply, no two people lead exactly identical lives, have exactly the same experiences. This means that no two people share exactly the same orientation, will communicate, interpret the world, or recreate their orientation in exactly the same way.

The most important consequence is the inevitability of social change, the result of the necessary imperfection of socialization. Members of the rising generation never absorb perfectly the orientation of their elders, and thus never recreate perfectly their social order. Reproduction is never perfect, and change occurs always. If you will, there is continual noise in the reproductive system, slippage which allows a slow but continual evolutionary change.

CONCLUSION

Burke's writings are extensive, and I have omitted from this brief discussion vast areas of his thought which deserve careful consideration because of the comprehensive and illuminating model of human action they contain. Particularly, I have passed over his discussion and incorporation of Freud, his substantive treatment of symbols, his extensive discussions of

the need for order and of symbols and symbolic actions as sources of cohesion and motivation, and his analysis of the formal aspects of language and its use, as well as his dramatistic method.⁸ (Overington [1977a, 1977b] discusses a number of these areas.)

In omitting these and other topics he treats, I do not mean to imply that they are insignificant. Rather, I think they rely on his model of knowledge and society, my concern in this paper. Without this model Burke's treatment of rhetoric and symbolic action—and more especially his treatment of the formal aspects of language—is isolated from an understanding of social and economic processes as they relate to symbol use and so becomes unacceptably idealistic. So it is proper to deal with his sociology of knowledge first.

Certainly Burke himself dealt first with the social bases of knowledge and language. Thus, in this paper I have relied on his earlier writings, particularly *Permanence and Change* and *A Grammar of Motives*. The extensive literary and philosophical analyses in these books and his detailed literary analyses of individual texts and authors contained in them should not blind the reader to their overwhelmingly sociological undercurrents. Only after laying the social foundations, only after securely linking the symbolic realm and the social order, does Burke give free rein to his interest in the formal, structural aspects of language and rhetoric, as he tries to relate much of human activity to the nature of language and its use.

Substantial elements of this interest appear first in his essay "The Philosophy of Literary Form," written in the late 1930s; he treats them in *A Rhetoric of Motives*; they come to the fore in his extended essay "A Dramatistic View of the Origins of Language," published in the early 1950s (and reprinted in Burke [1966]); and they dominate *The Rhetoric of Religion*, by which time he was prepared to say that language is the center of human motivation ([1961] 1970, pp. v-vi).

I see these later works and the interpretations of Burke based on them as resting on the more sociological early writings. Symbolic action in general and language in particular are important centers of human motive and certainly they deserve the elaborate treatment Burke gives them. After all, they do have a structure, limitations, and resources of their own. However, one needs to remember that they are powerful because they are rooted in the group and its mode of production, they are shaped by hier-

⁸ Burke discusses Freud in "Twelve Propositions" ([1941] 1973, pp. 305-13) and in "The Thinking of the Body" and "Somnia ad urinandum" (1966, pp. 308-43 and 344-58). He discusses sexuality, and particularly relations between the sexes, in his treatment of Shakespeare's *Venus and Adonis* ([1950] 1969b, pp. 212-21), which also is a good example of his substantive treatment of symbolic statements. He treats the need for order and the cohesive aspects of symbolism in "Order," part 3 of *A Rhetoric of Motives*. Part 1 of *Permanence and Change* is an extended discussion of symbols and their relation to motivation. His *The Rhetoric of Religion* is almost entirely a structuralist and formalist treatment of language.

archical relations within the group, and they are used to affect the course of group actions. Thus, his early works provide the sociological frame on which his later works rely.

Burke is concerned intensely with the symbolic, but as I have shown in this paper he sees the symbolic in dialectical interaction with the socio-economic. To focus on the power of the symbolic while ignoring its roots in social power and the mode of production, to focus on the creative use of the symbolic while ignoring the social and economic constraints on that use and on the interpretations of the products of that use, is to read only half of Burke, to ignore the dialectic between these things which is the basis of his concern.

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Max Weber's *Ancient Judaism*¹

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The paper examines Max Weber's *Ancient Judaism*, one of the least studied of Weber's works, and relates *Ancient Judaism* to Weber's other writings, to its background in contemporary German scholarship, and to the question of the nature of Weber's historical sociology. Special emphasis is given to (1) Weber's analysis of the early Israelite tribal confederacy, (2) the role of the priesthood in ethical rationalization, and (3) the contribution of secular historiography (especially that of Eduard Meyer) as well as biblical studies to Weber's analysis.

Max Weber's *Ancient Judaism* is an interesting but much neglected work in classical sociology. Although some individual concepts often associated with *Ancient Judaism*, such as "prophecy" or "charisma," now have common currency, the work as a whole has received little critical examination and exerted little influence.² Its topic—the historical development of the

¹ This paper was first produced for a graduate seminar conducted by Robert Alun Jones at the University of Illinois in spring 1980. Successive drafts have benefited from his encouragement and perceptive comments. The general understanding of Weber that initially prompted the paper owes a great debt to Norman Jacobs. Thanks also to Norbert Wiley and two anonymous *AJS* referees for helpful comments. Requests for reprints should be sent to Tony Fahey, St. Patrick's College, Maynooth, County Kildare, Ireland.

² The scarcity of critical reaction to *Ancient Judaism* is highlighted by the fact that Guttman's (1925) article stands to date as one of the few critiques which shows both an appreciation of Weber's sociological concerns and some familiarity with biblical history and scholarship (though Guttman, a historian of Jewish philosophy, was primarily neither a sociologist nor a biblical scholar). Another less penetrating early account, provided by Schipper ([1924] 1959), was first published in Polish in 1924. These have been complemented only recently, most directly by Raphaël (1973), less directly by Liebeschütz (1964), who deals perceptively with Weber's assessment of the contribution of Judaism to European civilization (see also Liebeschütz 1967, pp. 302–35; A. Weber 1965, pp. 469–81). The chapter on *Ancient Judaism* in Bendix's (1966) exposition of Weber's empirical studies is the longest and most detailed account available. Though excellent in its own terms—i.e., as a restructured paraphrase intended to make Weber's sometimes difficult work accessible to a wider audience—it provides no critical assessment and only a limited effort to locate the study of Judaism in the context of the rest of Weber's work. For the rest, Weber's characterization of the Jews as a "pariah people" playing the role of "pariah capitalists" in European economic history (Weber [1917–19] 1952, pp. 3–5, 336–55; 1978, pp. 611–15; [1924] 1927, pp. 151–52, 166, 263–65) has provoked the most interest and reaction—some of it indignant at the pejorative implications of those terms (Shmueli 1968, 1969; Oelsner 1962; Taubes 1966; Baron 1937, pp. 23–25, 297, n. 7; Mühlmann 1966, pp. 8–12; Agus 1978, pp. 135–38; Cahnman 1974; Mendes-Flohr 1976, p. 94). However important these critiques

religion of pre-Exilic Israel—is reason at once for its interest to some and its neglect by many. As part of Weber's historical sociology of the ancient Mediterranean world it analyzes a key phase in the background to modern Western civilization, but at the same time the breadth of historical vision which led it to do so distances it from the narrower historical consciousness of the mainstream of modern sociology. That the details of its subject matter interest only a minority, however, should not obscure the wider value of *Ancient Judaism* as important evidence of the overall structure and development of Weber's thought. *Ancient Judaism* belongs to the later period of Weber's life and was completed after the bulk of the formal analysis of *Economy and Society*.⁸ It thus links the background of mature *verstehendes* sociology with a concrete research problem—a combination too little sought for in present-day formalist analysis of Weberian method. The combination displays a methodological care and a breadth of substantive concern that qualify *Ancient Judaism* as a model of Weberian empirical research, certainly a better model than the narrower and more speculative *The Protestant Ethic and the Spirit of Capitalism* ([1904] 1958). This paper aims to focus attention on *Ancient Judaism* and to examine its place in Weber's overall work, its background in contemporary scholarship, and its significance as a reference point for an understanding of Weberian sociology.

THE ANALYSIS: EARLY ISRAEL

In *Ancient Judaism*, Weber's stated objective is the explanation of the "pariah" status of the Jewish communities of the Diaspora; in the achievement of that explanation, Weber's ambitious requirement is the analytical description and explanation of the development of Israelite religiosity: the "actually decisive" questions are "wherein the indubitable peculiarity of Israelite religious development lies" and "how that peculiarity is historically conditioned" ([1917–19] 1952, p. 428; [1924] 1927, p. 4, n. 1).

Weber's quest for the peculiarities of mature Judaism led him to trace their origins back as far into the early Israelite tribal past as the sources would allow. His analysis deals first with the social and political conditions of early Israel, the preurban seminomadic stages of Israelite tribal development. It revives a key idea that Weber had first presented in *The Agrarian Sociology of Ancient Civilizations* ([1909] 1976)—that is, the concept of the political organization of the Israelite tribes as a *coniuratio* or oath-bound confederation of peasants (Weber's term "*Eidgenossenschaft*" is as-

may be for later Jewish and European history, they show little interest in the substantive issues of *Ancient Judaism*, which deal primarily with pre-Exilic Israel and mention the "pariah" concept merely as a loose and not extensively argued metaphor.

⁸ The major part of the work was carried out in the fall of 1916 (Marianne Weber [1926] 1975, p. 593).

sociated with the Swiss tradition of sworn peasant confederacies). This confederation was held to be a secondary political type: it was dependent on neighboring, stronger urban cultures for its existence, partly as a source of cultural forms that were part of its structure, partly as an object of fear and hostility that gave the confederation its *raison d'être*. It lacked any permanent organs of government and acted as a unit mainly in times of military threat from outside. Its cohesiveness, which was frequently precarious, was based on the possession of a common culture, which in turn reflected its marginality to major civilization centers. As such, it was a political type common throughout antiquity and present also in the medieval Switzerland of the Holy Roman Empire. However, Weber pointed to an important distinguishing characteristic of the Israelite confederacy—its peculiar religious basis. Moses, considered by Weber to have been the main figure in the formation of the confederacy, is linked as a type to the great lawgivers of the ancient Mediterranean world (such as Solon in Athens), whose concern was to minimize social tensions arising from conflicts of interest in an increasingly monetized agrarian economy. But Moses gave his social legislation a distinctively sacral character by linking it to the requirements of a specific god (Yahweh). Thus the social and political integration of the tribes was given a definite religious tinge (Weber [1909] 1976, pp. 77–78, 135–43; 1978, pp. 442–43, 1202).

Ancient Judaism analyzes the religious foundation of the political organization of the Israelite confederacy—the concept of the “covenant” with Yahweh. The origin of the concept is examined by reference to tribal social and environmental conditions in which a purely practical notion of the covenant as “contract” (*b'rit*) is argued to have been a central mechanism in the arrangements which stabilized relations between settled tribes and the wandering artisans and merchants—“metics”—essential to the tribal economy. The relationship with Yahweh, in Weber's view, was conceptualized by the tribes with an imagery drawn from the everyday metic contracts common to tribal life.⁴

The covenant conception was regarded by Weber as the initial great uniqueness of Israelite religiosity and political organization: though tribal confederacies centered on a religious cult are found elsewhere (most evidently in the Delphic amphictyony in Greece—see Noth [1930], p. 53), nowhere else is the cult founded on a conception of a direct contract be-

⁴ Clarifying the logical status of this explanation, Weber explicitly rejected any “historical materialist” construction of his analysis as an attempt to represent the religious conception as an “ideological exponent” of its economic conditions; these conditions “codetermine” (*mitbestimmen*) the idea, but its origin as a specific event in history is “determined by quite concrete religious-historical and often highly personal circumstances and vicissitudes”; its social acceptance and utilization, however, as an instrument in the “selective struggle for existence against other less stable political organizations” is to be understood in terms of social life conditions (Weber [1917–19] 1952, pp. 79–80).

tween the community and its deity.⁵ Yahweh is uniquely thought of as an active partner in the social contract as opposed to the more widespread notion of God, as a witness and guarantor of the pledges and agreements men make among themselves. Flowing from this conception, the tribes' relation with Yahweh takes on a number of characteristics. Like the metics of daily experience, Yahweh is seen originally as an outsider, "a god from afar," not bound to tribal territory and not defined in terms of the essence of tribal social structure. The agreement with him, again as with metic guests, has a specific functional purpose—in this case a partnership for the purpose of making war and guaranteeing victory against Israel's enemies. The contracting of his services in this area by no means constitutes a claim to his exclusive recognition: the reality and might of the gods of other peoples are quite readily taken for granted, and even among the Israelites there exist a wide variety of cults and gods alongside Yahweh ([1917–19] 1952, pp. 139–48).

As the counterpart of Yahweh's commitment to his people, the confederacy incurred a set of obligations which in their content also became crucial to later Judaism. These obligations, conceived of as pleasing to Yahweh, were actually framed (in Weber's view, by the socially conscious Moses) to stabilize and protect the integrity of the tribal social structure: they sought to institute and sanctify mechanisms for reconciling or minimizing internal tensions, particularly to protect "free men against the consequences of social differentiation in wealth and power" ([1909] 1976, p. 136). They also sought to "maintain the good old patriarchal ways" by outlawing practices "unheard of in Israel"—such as the *corvée*—and by sanctifying resistance to urban or foreign bondage (Weber [1909] 1976, pp. 135–40; 1978, pp. 442–44; [1917–19] 1952, pp. 61–89).

⁵ In clear contrast to sociologists' preoccupation with Weber's comments on ethical rationalization, prophecy, and the pariah concept, biblical scholars refer almost exclusively to that section of his work on the social and political structure of early Israel—the confederacy and the early monarchical state. Caspari (1922a, 1922b, 1924) gave the first systematic consideration to some of Weber's theses in these areas. Following Caspari, Weber's work was taken up by Alt and Noth, the most innovative biblical historians of the 1930s. Alt ([1930] 1966, pp. 225–309) took direction from Weber's concept of war-leader charisma in his analysis of the early Israelite state formation and elsewhere spoke of the remarkable originality evidenced in Weber's work (Heuss 1965, p. 538). Noth (1930, pp. 20, 47, 91, 107, 113, 163), besides endorsing Weber's judgment on a number of specific historical details, developed Weber's analysis of the Israelite confederacy in a seminal work on the socioreligious structure of early Israel. A more recent trace of this use of Weber can be seen in Von Rad ([1959] 1962, pp. 1–34). However, despite these indications of Weber's ability to contribute to a difficult field in which he was no specialist, it is easy to exaggerate his impact on Bible studies. Hahn (1966, pp. 165–73) does so by claiming a Weberian legacy for any subsequent trace of a sociological approach. (Kimbrough [1972], taking issue with Hahn, sees in the work of the French scholar Causse a Durkheimian rather than a Weberian influence.) In general, the style of institutional analysis favored by Weber has had at best merely a tributary influence on the mainstream of Bible studies.

Weber thus regarded the covenant as primarily a sociopolitical instrument for the maintenance of the external boundaries and internal structures of the confederacy. His treatment emphasized the environmentally conditioned nature of both the political organization and its religious objectifications: these were predicated on the cultural influence of sophisticated neighboring urban civilizations, the simultaneous political independence of the tribes from these same civilizations, and the peculiar socioeconomic conditions of Palestine, which facilitated and conditioned the emergence of the covenant concept.

According to Weber, one cannot posit as a historical entity at any point an isolated, "primitive" Israel, uncontaminated by influence from advanced urban literary culture. On this point Weber passes brief critical comment on the evolutionary interpretation of Israelite religious development, which tried to find in early Israel "magical and 'animistic' representations observed everywhere among 'primitive' peoples . . . [which] must have been displaced only later by 'higher' religious conceptions" ([1917-19] 1952, p. 427). Weber acknowledged that analogies could be established between Israelite ritual and myth and those of magical and animistic representations elsewhere, but warned that overemphasis on these analogies could distort analysis by ignoring advanced cultural influence—particularly from Babylon.

These comments, brief though they are, set Weber apart from an alternative anthropological approach to early Israelite religion. His criticisms were directed mainly against William Robertson Smith, whose work on "primitive" Semitic religion was later considered by Durkheim to have had a revolutionary impact on the development of his own sociology of religion (Jones 1977; Beidelman 1974). Robertson Smith was the first major Bible scholar in the English-speaking world to appropriate late-19th-century German advances in biblical criticism and philology. These he combined with ideas from J. F. McLennan's totemic theories of primitive religion (first published in 1869-70) to develop a classical 19th-century evolutionist interpretation of primitive Semitic religion ([1899]1927). The most influential part of his analysis was his theory of sacrifice as a socially integrating communal meal. The emphasis of this theory on the totemic associations of Semitic sacrifice gained it scant respect in German circles—the notion was assaulted first by Nöldeke, whose critique was amplified by Meyer ([1906] 1967, pp. 308-11), whose view in turn was accepted without question by Weber ([1917-19] 1952, p. 427). In France, however, it reached the Durkheimian school through Smith's pupil and protégé, James G. Frazer—especially through Frazer's *The Golden Bough* (1890)—and had important influence in works such as Hubert and Mauss's "Essai sur la nature et la fonction du sacrifice" ([1909] 1929) and Durkheim's *The Elementary Forms of the Religious Life* ([1912] 1965).

Weber ([1917–19] 1952, p. 247) felt that Meyer had “rightly ridiculed those who wished to find evidence of ‘totemism’ in early Israel” and thus rejected this aspect of Smith’s work. However, his own treatment of Israelite sacrifice did not explicitly confront the general theory of sacrifice developed by Smith. This is not surprising. First, in *Ancient Judaism* Weber was primarily concerned not with general theory of any kind but with the explanation of concrete historical events. Second, Weber’s treatment reflects his view of the relative unimportance of sacrifice in Israel’s religion: in principle, “people did not worship God by offering sacrifice,” and in practice “the fulfillment of [God’s] *b’rit*-sanctified commandments [was] at least of equal or actually greater importance than occasional sacrifices offered by individuals and later by kings and temple priests.” Thus “sacrifice in ancient times simply could not gain the significance in relation to Yahweh which it obtained elsewhere” (Weber [1917–19] 1952, pp. 135–36). Weber accepted the view that in many cultures, sacrifice had important social integration functions, the central point of Smith’s theory (Weber 1978, pp. 423–24). In Israel, however, he considered sacrifice as mainly supplicatory and, in later development, as expiatory.

Weber’s analysis of the confederate period was geared to isolating and explaining items with unique cultural significance for subsequent history, in this case, intellectual productions embodied in tradition that gain crucial importance in later religious developments. The earliest manifestations of the confederate religion exhibited one such central item—a distinctive *ethical* religious orientation. This was formed from the idea of a mutual agreement between God and his people (viewed as a political unit), which had to be honored by both sides. From the beginning, this made possible a progressive reduction in the symmetry of the agreement, which elevated the deity and transformed the mutuality into a one-way flow of directives from God demanding obedience. But even from the beginning the idea of compliance with rules (ethics), as opposed to magical coercion, was a central element of the covenant relationship.

MONARCHY, URBANIZATION, AND RELIGIOUS TRANSFORMATION

The bridging of the historical gap between the simple Yahwism of the early tribes and the majestic monotheism of the classical prophecy is the most complex strand of *Ancient Judaism*. This part of the analysis, usually overshadowed in secondary commentary by the later issues of prophecy and the Jewish pariah community, in fact is the major section of the whole work.⁶

⁶ The discussion of classical prophecy, commonly taken to be the central theme of *Ancient Judaism*, actually occupies only two of the 14 chapters in the English translation of Weber’s original published work (the inclusion of the later fragment on the Pharisees reduces the proportion devoted to prophecy even further). Weber’s evident

Whereas the treatment of prophecy focuses on a single already well-known topic and simply adds to it a peculiar Weberian sociological tinge, this section builds on the entire background of Weber's conceptual development and draws from it some of the central themes of his world-historical analysis. In analyzing the immediate preprophetic phase of Israelite religious development, Weber sets out to clarify the nature of the Israelite city (as economic and political structure), the semipatrimonial Israelite monarchy, and the official and nonofficial Yahwist priesthoods. In the process he utilizes major sets of typologies contained in *Economy and Society* and locates Israel, in terms of its social and religious formations, within the tripartite typology of his preindustrial world structure: Mediterranean antiquity, medieval Europe, and the age-old Oriental empires.

The central developments in social structure following the decline of the peasant confederacy were centralized monarchy and urban organization among the Israelite tribes. The defeat of the Israelites and the occupation of Palestine by the Philistines in 1050 B.C. signaled the political and military obsolescence of the confederacy and brought its demise. Under Saul (1020–1000) and David (1000–961) began the drive toward a more institutionalized and centralized organization of the tribes, impelled first by the military exigencies of the struggle with the Philistines. With the successful expulsion of the Philistines the leadership of David was transformed into a hereditary kingship, legitimated on the basis of David's personal charisma and the prestige attached to his success as war leader.⁷ This innovation hastened the decline and subjugation of the free peasant tribes. Under the reign of Solomon (961–922 B.C.), the monarchy began to develop a rigidly structured state administration, modeled on the patterns of the Egyptian patrimonial order (Weber [1917–19] 1952, pp. 98–101).

A more gradual but nonetheless fundamental social transformation took place with the slow penetration of the Israelite tribes into the Canaanite cities of the Palestinian plain. Powerful individual clans, growing rich on possession of land and revenues from caravan routes, began to emerge as a powerful stratum of knights and drove toward the formation of Mediterranean-style urban centers. By Solomon's time the powerful urban "patriciate" was a central feature of Israelite social structure, exerting, on the one side, an oppressive domination over indebted and cliented peasants in the countryside and, on the other, a competitive check and limitation on

personal sympathy with the dramatic heroism of the prophets (Hufnagel 1971, pp. 354–59; Steding 1932, pp. 31–47) should not be allowed to obscure his extended analysis of the less colorful historical preconditions which made prophecy possible. Careful examination of this analysis will help redress the overemphasis on the "prophetic breakthrough" as an element in Weber's view of social and religious change. (This emphasis is particularly evident in Parsons [(1949) 1968, p. 567; 1964, pp. 29, 33–35].)

⁷ An instance of "routinization of charisma" by the charismatic individual within his own lifetime.

the authority of the monarchy in the cities (Weber [1917–19] 1952, pp. 14–23, 53–56; 1978, pp. 1228–30).

The relative weakness of the monarchy in its failure to dominate completely the urban notables restricted its patrimonial character and was due partly to economic conditions. The emergence of the Israelite variation of Mediterranean urbanism had as a precondition the absence of the normal economic base of a powerful patrimonial order—that is, the great inland riverine agricultural systems such as those of neighboring Egypt and Mesopotamia. The fertile serried valleys and high mountain plateaus of Palestine, to which the long narrow plain was but a coastal fringe, precluded the irrigation and water-control systems whose management demanded the centralized bureaucracy of the typical Oriental patrimonial order. Topography and early social structure thus conjoined in positioning the developed Israelite polity as a transitional case between the Mediterranean polis and the Near Eastern “despotism,” with the resultant contradictions acting as the fountainhead of a unique cultural creativity.

The intellectual and religious developments that emerged from the social ferment underlying the monarchy were the central concern of Weber's analysis. These developments, far from being a progressive march onward and upward toward ethical monotheism, corresponded in their complexity and convulsiveness to their tension-ridden social background. Civil war, religious persecution (as under King Ahab's consort Jezebel, champion of the Tyrean Ba'al cult), factionalism within the Yahwist priesthood, threats of submission to royal sultanism, penetration by foreign and domestic ritualism—all combined, successively and conjointly, to wrench Yahwism along a jagged and uncertain path. Weber's analysis at this point is at once complex and hesitant—the tumultuousness of the events and the sparseness of the sources allow neither simplicity nor firmness in specific conclusions. The crucial actors in religious development—the Levitical priesthood and the vaguely defined “lay circle of Yahwist intellectuals”—appeared to Weber in different forms and with different motivations in different parts of the sources. At one point, they focus on the official temple cultus and in typical theocratic fashion attempt to infiltrate the state apparatus and to exercise ideological control over the kingship. In another aspect they struggle to establish the cultic supremacy of the Jerusalem temple against local Yahwist centers and their attendant priests. Elsewhere, the antagonism to traditional magic or charismatic ecstasy, or the taking of common political cause with the deprived peasantry, the aristocratic patriciate, or the monarchical authority reveal the shifts, the inconsistencies, and the multidimensionality of the Yahwist champions.

However, from this complexity, Weber picks out one strand as relatively continuous and decisively important for the intellectual and ethical maturation of Yahwism. This is the success of the Levitical Torah teachers—as

opposed to cultic officials—in meeting the emotional and religious needs of the masses with a sophisticated system of pastoral care. Release from guilt through the recognition and expiation of sins pushes to the fore the idea that misfortune is the result of offense against God or transgression of his will. The soothing of individual angst by this means gains enormously in popular appeal with the professionalization of a pastoral priesthood. The priesthood, with its specialized means of knowing the divine will, lends its aura of authority to the identification of offensive deeds and to the specification of expiatory acts which will succeed in regaining divine favor. The securing of a popular following for this service contains within it a strong religious dynamic: on the one hand, magic—the ritual coercion of the deity—along with individual charismatic authority, is combated as inimical to the exclusiveness of the priests' access to God and as hostile to God's transcendent superiority; on the other hand, the ever-increasing complexity of moral problems requires an increasing sophistication in the ethical casuistry applied to the deciphering of God's will. Elaborate moral arguments, tailored to fit the problems and consciences of the priestly clientele, become the stock-in-trade of the priestly craft and lead to the accumulation of a dogma, a canon, and an ever-refined image of the divine nature (Weber 1978, pp. 422–33, 437–39, 452–67; [1917–19] 1952, pp. 178–83, 215–25).⁸

Priestly rationalism, confined as it was to the requirements of a popular service for which fees in some form were expected, had a strictly limited range. Yet, contrary to the common image of the ritualist, cultic orientation of the priesthood, Weber saw it as a vital link in the chain of development of Judaic ethical rationality. It defended and carried forward the key concepts of the early Yahwist tradition. In doing so it refined and clarified those concepts, affirmed obedience to ethical rules, and aggressively disclaimed magic in man's relations to God. Though the priesthood was

⁸ Weber's analysis of the pastoral character of the Levites has been rejected by Guttman (1925, p. 216) as without foundation. Otherwise, to my knowledge, biblical scholarship has devoted no attention to this particular thesis. Weber regarded pastoral rationalization as a fairly recurrent element in the world religions, Israel being in this regard a particular instance of a widespread phenomenon. Recent biblical scholarship has begun to uncover in the "wisdom" literature of the Old Testament (mainly in Proverbs, Job, Ecclesiastes, and some of the Psalms) a religious institution which has some resemblance to Weber's pastoral priesthood. Weber himself ([1917–19] 1952, pp. 197, 228, 285) accepted the contemporary view of the relative unimportance of wisdom in pre-Exilic Judaism. However, this view was undermined in the 1920s (see Rankin [1936] 1969, pp. 6–7), and now wisdom teachers are regarded as a sacral institution ranking alongside the cultic priests and the prophets in Israel's religious history. Also, more emphasis is placed now on the pastoral and rationalizing characteristics of the wisdom tradition: its concern for individual religious need, its institutional separation from priesthood but intellectual ties with developing Yahwism, its increasing concern for sophistication and refinement of religious concepts, and its rationalizing hostility to magic and empty ritual (Eakin 1971, pp. 164–81; Rankin [1936] 1969, pp. 53–76; Von Rad [1959] 1962, pp. 383–453). However, how Weber's analysis might be altered by these developments is beyond my competence to judge.

geared primarily to the needs of the individual and indeed often to the great and the powerful, the social diversity of its clientele and its essential independence from the claims of the authorities freed it to retain in its outlook strong traces of explicit political criticism—a reproachfulness and disapproval of the luxurious ostentation of the Jerusalem court and a condemnation of the dalliance with foreign gods and the institution of practices by an overbearing state that were “unheard of in Israel.”

This interpretation of the Levitical pastoral priests reduced the opposition between prophecy and earlier priestly religion so common to Protestant biblical scholarship of Weber’s time. It led to the view that prophecy was drawing on rather than controverting an aspect of the priestly tradition and thus that it did not independently make the jump from the simple tribal concept of a strictly limited Yahweh to the omnipotent universal god of the prophecies of doom (Weber [1917–19] 1952, pp. 304, 332–34). Berger (1963) has devalued Weber’s view of this issue by equating it wholly with a sometimes prevalent Protestant view of the absolute hostility and separation between prophet and priest. The ascription of this view to Weber is not wholly without foundation: in *Economy and Society* Weber generalizes rather sweepingly on the tensions and oppositions that inhere in the relative positions of priest and prophet (1978, pp. 439–42). However, in *Ancient Judaism* the relationships specific to Israel are described more carefully: the variations in prophetic attitudes and positions relative to the cult are fully recognized. Isaiah is said to have been “closely befriended by distinguished priests,” Ezekiel is recognized actually to have been both priest and prophet and Jeremiah to have been descended from a landed priestly family ([1917–19] 1952, pp. 277, 366). Though individual priests are treated with great hostility, nowhere is the Temple or its cult condemned as a whole; indeed, with Amos the fight is against Baalite impurities creeping into the cult rather than against the cult itself (p. 283). Furthermore, Weber points to a lack of homogeneity in the priesthood itself: the pastoral section of the priesthood, as already noted, is shown to be hostile to ritual emptiness or dishonesty and to be in close sympathy with prophetic denunciations (p. 217).

PROPHECY, POLITICAL DISASTER, AND THE ESTABLISHMENT OF THE JEWISH CONGREGATION

The outlines of Weber’s treatment of prophecy are too well known to require presentation here. In his analysis Weber sticks to the main lines of the arguments standard in Old Testament scholarship: the potency of the prophetic pronouncements is attributed to the passion and eloquence of their presentation in conjunction with the catastrophic political consequences of the Assyrian and Babylonian invasions. With the condemna-

tions of Israel's sinfulness and the promise of doom as a punishment for faithlessness, the trauma of political disaster is pressed into service by prophecy as the ultimate and overwhelming proof of Yahweh's majesty and justice. The destruction of Jerusalem and the national state is interpreted as a divinely ordained chastisement designed to bring back the chosen people to their former loyalty. This conception consolidates Yahweh's monotheistic character: it elevates him to the level of god of the universe, manipulator of empires, a majestic divinity who plays with history to chastise his people. The exile in Babylon crystallized the distinctive eschatology of hope of subsequent Judaism. God's punishment having been completed in the fall of Jerusalem, the message of doom could give way to the message of hope espoused by Ezekiel and Deutero-Isaiah. Thus development resolves the paradox of the Jewish congregation's pariah status: the occurrence of disaster confirms rather than denies God's justice. It strengthens his people's faith in his promises for future political salvation and bonds them together in their present political degradation.

Weber's analysis of prophecy does not achieve the same level of originality as the earlier sections of his work, though as an assessment and synthesis of the best in contemporary literature his treatment is balanced, insightful, and sure of judgment. His main concern in this analysis was to re-examine prophecy as a sociological type—that is, to specify its religious character and at the same time locate it against its social background as a not entirely individually determined phenomenon. In typical fashion, Weber did this by a series of comparisons, contrasts, and juxtapositions that attempted to define the various facets of the prophetic character and its social role. He concludes that prophecy is the result both of the dynamic that emerged from the prophets' own highly varied "psychic dispositions" and of the "great sounding-board of the world-political stage of their times" (Weber [1917–19] 1952, pp. 307, 285). Without the turmoil of international war, the weakness of local state apparatus (seen, e.g., in the lack of an adequate system of state censorship of the state's outspoken critics), and the traditional cultural attitudes of the Israelite people—all bound up with a range of additional factors—Israelite prophecy is inconceivable.

The major service of Israelite prophecy to world history was the consolidation and elevation of the strand of ethical orientations already clearly laid down in the Levitical Torah (Weber [1917–19] 1952, p. 304). Prophecy endowed those orientations with an extraordinary vigor—sufficient to carry the Jewish people intact through the great disasters of imperial subjugation—but it did not itself create them. In Weber's time Protestant theology, in keeping with its Reformation heritage, tended to exaggerate the novelty of the prophetic message: it viewed prophetic ethical teaching through Pauline spectacles—that is, on the assumption that the prophets, like Paul, broke through to thinking in terms of an absolute tension be-

tween good and evil, setting the weaknesses of the flesh against the purity of the spirit (Von Rad [1959] 1962, pp. 370–71). However, for Weber “ethical behavior” refers to a much broader and simpler category: it means simply behavior carried out in obedience to a religious rule. It is opposed to magic, which is founded on the belief that certain actions or words can compel or manipulate divine powers to act in a particular required way. In the elementary form, ethical rules require no coherent distinction between good and evil as general principles: like magic ritual, these rules are “frequently composed of a complex of heterogeneous prescriptions and prohibitions derived from the most diverse motives and occasions . . . [with] little differentiation between the important and unimportant requirements: any infraction of the ethic constitutes a sin” (Weber 1978, pp. 437–38).

It is clear from Weber’s analysis of prophecy that he viewed prophetic rationalization as going partway along the road to distinguishing important from unimportant ethical requirements; but clearly also he did not see the prophets as creators of the Pauline view of sin as the “unified power of the antidivine.” The prophetic mission was to demand absolute obedience to God’s will as outlined in the Levitical Torah. God’s requirements were presented as binding simply because God required them. They were not thought of as part of an eternal, morally correct ethical order: in fact, they sometimes appeared to the prophets as amoral or evil in the naturalist sense of cruel, deceitful, destructive (Weber [1917–19] 1952, pp. 309–10). Thus prophetic ethical rationalization exalted the notion of obedience to God’s expressed will—but it did not ruminate philosophically on the universal characteristics of that will. In this sense it lacked the conceptual basis on which to assert the inherent evil or sinfulness of ritual: whereas Paul and the Puritan reformers condemned ritual as illusion—or worse, as pride in the sinful flesh—Israelite prophecy could and did judge it primarily only on the evidence of its compatibility with God’s changeable will.

ANCIENT JUDAISM IN WEBER’S SOCIOLOGY

Weber published his studies of Judaism, the religions of India, and the religions of China under the general title “The Economic Ethics of the World Religions.” This title, though it does not convey the breadth of material covered by the Indian and Chinese studies, does correctly specify a major focus of interest in these two works. This is not the case with *Ancient Judaism*, which shows no interest in the consequences of Israelite religion on the economic life or thought of Israel. *Ancient Judaism* very explicitly has a different focus—the specification and explication of those characteristics of Judaism which enabled the formation and preservation of a geographically dispersed and ritualistically segregated Jewish congregation after the destruction of the states of Israel and Judah. This congre-

gation fell to pariah status in its surrounding world, but in spite of this—or rather, as Weber felt, partly because of it—retained its solidarity and religion intact. For Weber, this development, which seemed so paradoxical, was “unique in the socio-historical study of religion” and thus intriguing in its own right. In addition, it had world-historical significance: apart from the economic importance of the Jews in the European medieval and modern world, Jewish religion made available to Christianity the Old Testament as a sacred book and became “the instigator and partly the model for Mohammed’s prophecy” (Weber [1917–19] 1952, pp. 4–5). This long-term and many-faceted historical impact gave ancient Judaism its “value relevance” for the modern researcher. In addition, for Weber, as Liebeschütz (1964, p. 55) has suggested, there is an element of personal fascination which goes beyond purely scholarly objectives—“an interest in the Bible and the fate of religious thought represented in it which seems to come from a different layer in the scholar’s personality from the thesis he intends to prove and the concepts of his discussion.”

How then does this problem fit in with the rest of Weber’s work? Given his overall preoccupations, one can easily be tempted to interpret *Ancient Judaism* solely as a study of ethical rationalization—an interpretation which Tenbruck (1975) feels is too lightly applied to many of Weber’s works. Such a summary characterization is harmless as long as rationalization is not so narrowly defined as to obscure some of the very particular questions in this study—why Israelite ethics were concerned with communal and political behavior more than with individual behavior, why their promise of salvation is directed to this world rather than the next, how the emergence of the covenant concept was related to the sociopolitical conditions of Palestine, and so on. One can interpret these questions as feeding into the study of rationalization; but at the same time, one must remember that in *Ancient Judaism* Weber himself saw them as also feeding into the specific historical question of the development of the Jewish congregation.

The search for themes in *Ancient Judaism* common to Weber’s other work can distract from that work’s specific character. Rather than rely excessively on such a search, one can specify the link between *Ancient Judaism* and the other writings in a different way, that is, in terms of his frequently stressed logical distinction between two possible usages of empirical data—historical explanation (causal analysis of specific developments) and abstract concept formation. Weber’s interest in the religion of Israel not only is reflected in *Ancient Judaism* but also penetrates sections of *Economy and Society*, especially the chapters on the sociology of religion and, to a lesser extent, those on the sociology of law, the sociology of domination (with reference to hierocracy), and the city. The political, economic, and religious characteristics of the early Israelite confederacy are also objects of interest in *The Agrarian Sociology of Ancient Civilizations* ([1909]

1976).⁹ The references in these works show that materials from Israelite sources were an important empirical reference in the construction of some of Weber's central general concepts—charisma, priesthood, prophecy, ethical and legal rationality, hierocracy.¹⁰ On the other hand, the same material is used in *Ancient Judaism* with an explicit explanatory focus and with reference to concepts as instruments rather than as ends in themselves.¹¹ This dual usage of empirical material is typical of all Weber's substantive work and reflects his long-standing belief in the logical alternatives always present within science. In his methodological debate with Meyer he referred to "the absolutely fundamental logical distinction" between "(1) conceptualization with the illustrative use of 'particular facts' as typical instances of an abstract 'concept' . . . and (2) integration of the 'particular fact' as a link, i.e., as a real causal factor, into a real, concrete context—with the use . . . of the products of conceptualization . . . as exemplificatory and . . . as heuristic devices" (Weber [1904] 1949, p. 135). Though "it always happens . . . [that] these two absolutely distinct standpoints become intertwined in the practice of the student of culture" (p. 136), it is clear in the present instance that in the conceptual work of *Economy and Society* references to Israel approximate to the first activity and in the historical study of *Ancient Judaism* to the second. *Ancient Judaism* and *Economy and Society* are thus the two halves of a single enterprise, logically separable but in practice thoroughly interdependent.

The treatment of Israel in *Ancient Judaism* focuses on specific historical problems and thus should not be seen only as an effort to contribute to some more general line of thought. The appearance of some of the same material in *Economy and Society* shows, however, that such a general contribution can be drawn. Those who emphasize Weber's view of sociology as the "handmaiden of history" (Mises 1929, pp. 471–84; Cahnman 1964, pp. 119–20; Heuss 1968, pp. 50–51; Roth 1971, pp. 253–65) need no bet-

⁹ The references to Israelite material in *Economy and Society* and *The Agrarian Sociology of Ancient Civilizations* are indicated in my outline above of *Ancient Judaism*: nearly all major references to *Ancient Judaism* are accompanied by parallel references to one—and often both—of the other two works.

¹⁰ The contribution of this material, however, should not be exaggerated. Even the ideal type of "ethical prophecy," which sounds so similar to the reality of Israelite prophecy, is exemplified best, in Weber's view, by Zoroaster and Mohammed (1978, p. 447).

¹¹ Nowhere in *Ancient Judaism* does Weber make explicit reference to *Economy and Society*, but the dependence of the former on the latter as a conceptual reference manual is undeniable. Certain key arguments in *Ancient Judaism* (on the Israelite city as Hellenic "polis of the gentes," the Solomonic kingdom as a "patrimonial-bureaucratic corvée state") use a terminology that is clarified properly only in *Economy and Society*. On a broader level, such themes as the political tensions between urban patriciate and central bureaucracy and between urban patriciate and the peasantry, or the priesthood as a rationalizing force in religion, are examined in *Economy and Society* as general processes occurring in various forms in different historical circumstances.

ter example than *Ancient Judaism* and the use it makes of general concepts. On the other hand, those who evaluate *Ancient Judaism*, and indeed the other studies of world religions, in terms of their contribution to the scholarly apotheosis of *Economy and Society*—history serving sociology—are not wrong either (see Tenbruck 1975). The caveat must be entered, however, that in the case of *Ancient Judaism* at least such a contribution is not explicitly developed in the primary study but is dealt with only elsewhere—mainly in *Economy and Society*.

A recurrent curiosity about *Ancient Judaism* concerns its connection with issues raised in *The Protestant Ethic and the Spirit of Capitalism* (Wax 1960; Berger 1963). This curiosity may be due to the prominence of *The Protestant Ethic and the Spirit of Capitalism* in popular awareness of Weber's work and the consequent tendency to see all his other works in its light. It is not warranted by any clear-cut substantive continuity between the two that would identify them as a pair among the rest of his works. One should certainly not speak of Weber in the same context as those who saw Judaic prophetic religion as "the Protestantism of antiquity" (Berger 1963, p. 944). Taken too seriously, such analogies were anathema to Weber; used carefully as illustrative contrasts and comparisons, the references to Protestantism were no more and no less useful than any other comparative references Weber makes (in *Ancient Judaism* they are actually a good deal less prominent than his references to the religious institutions of Egypt, Babylon, and even India).

Elsewhere, explicitly raising the problem of the comparability of Puritanism and Judaism, Weber warned against oversimplification—in making such comparisons, it was necessary "not to think of Palestinian Judaism at the time of the writing of Scriptures, but of Judaism as it became under the influence of many centuries of formalistic, legalistic and Talmudic education" (Weber [1904] 1958, p. 165). Even when Puritans felt an "inner similarity" to Judaism, Weber pointed out that they were also aware of the "limits of that similarity" and the degree to which they rejected or altered aspects of the Judaic heritage, including its economic ethics (Weber 1978, pp. 622, 623; see also [1904] 1958, pp. 270–71). The strongest substantive similarity Weber found between Puritanism and Old Testament thought lay not in the words of the great prophets but in the images of God and of faith in the later and more modern-sounding Book of Job (Weber [1904] 1958, p. 164). Certainly an interpretation of *Ancient Judaism* as a search only for the causal antecedents of Protestantism, or indeed of any later historical development, would do no justice to the complexity and individuality of that work. ("A history of antiquity which would include only what exercised causal influences on any later epoch would appear . . . as empty as a 'history' of Goethe which . . . described only those

elements among his characteristics and his actions which remain 'influential' in literature" [Weber (1904) 1949, p. 154].)

Thus we can characterize *Ancient Judaism* as a single study of a very specific historical development, which is related to the rest of Weber's work through its instrumental dependence on the conceptual apparatus of *Economy and Society* and, more indirectly, through the provision of empirical reference material used in constructing certain parts of that apparatus. As both historical analysis and conceptual reference, the study in turn is developed as part of the program of world-historical analyses of Western and Islamic civilizations.

THE BACKGROUND IN CONTEMPORARY SCHOLARSHIP

If a proper appreciation of *Ancient Judaism* can be fostered partly by positioning the book in relation to the rest of Weber's work, it helps equally to have an awareness of the contemporary intellectual environment from which Weber developed the problem of Judaism in the first place. The political controversy on the contemporary "Jewish problem" had long penetrated German academic life, affecting all major German historians (Liebeschütz 1967) and reflected especially in Sombart's *The Jews and Modern Capitalism* ([1911] 1913). Weber's pronouncements on academic integrity and the duty not to use the lecture hall as political pulpit were directed partly against recurrent outbursts of anti-Semitism among his colleagues. But his concern with contemporary anti-Semitism, however strong, was at most only remotely connected with *Ancient Judaism* (anti-Semitic ideas are more directly confronted in *The Protestant Ethic and the Spirit of Capitalism*).

A more immediate—and, in late-19th-century Germany, a no less controversial—background factor was the intense interest generated by recent developments in German biblical scholarship. These developments, which had been rapidly accumulating since the mid-19th century, revolutionized accepted views on the historical character of the Old Testament and scandalized the faithful of the Christian world.

Protestant biblical scholarship in 19th-century Germany was inspired most of all by the contemporary spirit of new "scientific" history. In Bible studies, this spirit required that biblical texts be subject to the same critical evaluation as that which secular historiography applied to its sources before admitting them as historical documents. This approach, though skeptical of dogma, for the most part was not iconoclastic. Its aim rather was to reconcile the traditional view of these texts as divinely inspired with the increasing rationalist curiosity about the identity and contribution of the human authors who committed them to paper. In Old Testament studies, the leading controversy concerned the authorship of the Pentateuch, the first five books of the Old Testament, traditionally held to have been writ-

ten by Moses. As early as the 9th century A.D., occasional critics had pointed to internal contradictions, linguistic variations, and chronological impossibilities (references to events that postdated Moses) that cast doubts on the sole authorship of Moses (see Bratton 1959, pp. 299–301). These doubts, however, were suppressed by Christian dogmatism until the Reformation and the rise of Renaissance humanism opened the way to a spirit of freer critical inquiry. In the 17th century, rationalist thinkers such as Hobbes and Spinoza revived questions about inconsistencies in the traditional view. Spinoza particularly advanced the idea of the Pentateuch—and some other Old Testament books—as the work of editors, culminating with Ezra, who put these texts together from a number of distinct sources or documents (Bratton 1959, pp. 307–9). The 18th-century Enlightenment gave a further boost to the critical regard for the close reading of texts and added to the stock of observations which challenged traditional views.

However, these early suggestive indications of greater complexity in the construction of the Bible than was hitherto supposed remained inconclusive until 19th-century progress in philology, archaeology, and comparative religion opened new realms to biblical scholars. These enabled an accumulation of evidence, partly from the Bible, but increasingly from nonbiblical sources, which allowed a more detailed separation and dating of biblical documents. Thus Hupfeld (1853) postulated three distinct authors in Genesis. Following him Graf (1866) and Kuenen (1869–70) gave one of these a post-Exilic dating. The culmination of this flurry of discovery was reached in the work of Julius Wellhausen (1844–1918), who combined a gift for detailed research with the ability to recast a vision of the Bible in liberal 19th-century terms without sacrificing its sacral character. Wellhausen identified four documents in the Pentateuch (see esp. Wellhausen [1878] 1885). He dated these from the 9th to the 5th century B.C. and located the final compilation in the post-Exilic period (thus establishing that what was traditionally believed to have been the earliest was actually among the latest of biblical documents). He also interpreted these documents as reflecting a gradual evolution in Israelite religiosity—from simple animism through polytheism to monotheism. This interpretation appropriated parts of the terminology of contemporary Hegelian evolutionism and used it to redefine divine revelation as a gradual, cumulative manifestation embodied in the very course of history. This *Heilsgeschichte* notion was not unique to Wellhausen. But his development of it as an ordering framework for his precise and coherent textual analyses of the Pentateuch gave his arguments an extraordinary power. It was with the advent of his work in the 1880s that the new biblical criticism finally overwhelmed traditional teaching and established an orthodoxy that has survived in its basic outlines to the present day.

Because Weber possessed only a limited command of Hebrew, his study

of ancient Judaism depended heavily on the secondary analyses of Wellhausen and his followers, the range of which he mastered to an outstanding degree (Guttman 1925, p. 195). Weber ([1917–19] 1952, p. 426) acknowledged that Wellhausen brought the methods of critical analysis to “the highest systematic perfection” and that, even to Weber’s time, all Old Testament scholarship was still based on Wellhausen’s work “even when deviating widely from its conclusions.”

However, although biblical scholarship and its achievements were undoubtedly preconditions of *Ancient Judaism*, it is only in a very limited way that that work can be seen as following the biblical scholarship tradition. The setting of that tradition—its commitment to the assumptions of Christian theology, its explicit evolutionist assumptions, its exclusive concern for religious development, its disdain for historical consequences outside the development of Christianity—distanced it from Weber’s broader concerns. Weber’s dependence on that tradition was mainly instrumental. It provided the data on which he worked and no doubt in that capacity set limitations on and gave some shape to his analysis. However, the viewpoint from which he looked at the data and the questions he addressed to them reflect a different approach.

This approach is more strongly linked to the tradition to which he traced a large part of his intellectual genealogy—that is, the tradition of secular German historiography. That tradition from the early 19th century had a background of interest in the contribution of Judaism to Christianity—though much of that interest (as, e.g., in Hegel’s writings on the philosophy of history) reflected a hostile bias against all domineering religious institutions, especially those that could be held in any way responsible for the dogmatism and obscurantism of the Western Christian church. As Leibeschtütz (1964, p. 41) points out, from Spinoza to Hegel “there was a broad agreement between prominent representatives of the Enlightenment in England, France, and Germany that the Old Testament and its story of the chosen people was the root from which the ‘sectarian’ concept of a privileged truth had come to Christianity,” a heritage which “was considered the main barrier between the humanitarian ideas of modern man and the religious traditions of Europe.” The substitution of scholarly research for this blanket of narrow criticism took place only slowly. The first break came with Ranke’s recognition of the wider historical significance of Judaism, as he proclaimed it in his programmatic outline of the scope of *Universalgeschichte*: “The process by which Israelite religion obtained the supremacy over all other forms of religious worship and became one of the fundamental bases both of Islam and of the Christian world, forms one of the most important elements in universal history” (quoted in Butterfield [1955, p. 123]). The elucidation of the *Universalgeschichte* approach achieved partial fulfillment in Mommsen’s chapter on the Jews and Judaea

in his *Römische Geschichte* (1854–56). But it was brought somewhere near Ranke's original intention only in the work of Eduard Meyer, the last of the great German universal historians and a major reference in all Weber's work on antiquity.

Meyer is by far the most frequently cited authority in *Ancient Judaism*. Weber had a long-standing acquaintance with Meyer's work (his critique of Meyer's general discussions of historical method dates from 1905 and shows a familiarity with Meyer's work on Israel), and thus it is possible that Meyer provided Weber with his introduction to the specialist literature on Israel. Meyer's place in the background to *Ancient Judaism* along with his importance in Weber's methodological discussions (Weber [1904] 1949, pp. 113–88) and in *The Agrarian Sociology of Ancient Civilizations* thus gives him a considerable though little-noted position in Weber's intellectual environment.

From his earliest student days in the 1870s to his death in 1930, Meyer devoted his energies with a unique perseverance and singlemindedness to one massive project—the history of the ancient world from the earliest Egyptian records to the rise of Christianity and the decline of the Roman Empire. The main organizing principle in his work was the doctrine of the “unity of antiquity,” which insisted on the necessity of viewing each stage of ancient history as the product of the “unity of the whole” (Ehrenberg 1931, p. 502). This insistence was directed against the tendency among other historians to project contemporary regionalizing assumptions back into antiquity and treat the history of each place and period—be it Ptolemaic Egypt, Periclean Athens, or the Roman Empire—as a separate story requiring a separate academic subdiscipline. To realize his program, Meyer mastered the languages of the ancient Mediterranean and Near Eastern literary cultures and immersed himself in their original documentary records, attempting to incorporate within himself the means to a panoramic vision of the historical sweep of the entire ancient world. (For a short account of Meyer, see Liebeschütz [1967] and Ehrenberg [1931].)

Meyer's own monument to his work was his five-volume *Geschichte des Altertums*, published in segments and frequently revised between 1884 and 1930. In that work and in additional supplementary publications (Meyer 1896, [1906] 1967, 1912) Meyer dealt extensively with Israelite and Judaic history within its world environment. Though he dealt extensively with the work of biblical scholars, he approached Israelite history not as a stage in God's revelation of himself to mankind, but as a phase of the world history of antiquity, in which Israel's role had to be estimated in the same spirit as that of the Egyptians or the Babylonians, the Greeks or the Etruscans. Meyer's treatment of that role focused on one theme that reoccurs strongly in Weber's work—the paradox of Israel's distinctive contribution to Western civilization from the midst of a thoroughly Eastern polit-

ical and cultural environment. Meyer was by no means the first to see Israel and its culture as a juxtaposition of elements from East and West (see Shaffer 1975), but unlike his predecessors (who were mainly in biblical criticism) he moved beyond analyses of literature and religion to define those elements in terms of socioeconomic and, more important, political transitions. Early tribal Israel he considered in many ways proto-Hellenic—its politics tending toward Greek-style democracy, its historians (especially the authors of the Song of Deborah) comparable to Herodotus (and far superior to those of neighboring Egypt, Assyria, and Babylon [Meyer (1906) 1967, p. 486]), its early prophets (Amos especially) comparable to the poet-philosophers of the Greek epics (Hesiod in particular). Later Israel, however, was politically transformed, first by internal state formation (beginning with the Omrid monarchy), then by imperial subjugation, and moved in the direction of its Near Eastern neighbors. With that began the stifling of Israel's early cultural promise—smothered by state authoritarianism and its hierocratic ritualism. So too began the struggle of the Yahwist Levites and later of the great prophets against the transformation. In the crucible of this struggle the religiosity of Yahwism gained its distinctive shape—a political and religious utopianism that, in the face of the oppressive realities of external and internal political developments, harked back to the golden age of tribal simplicity and its God-ordained social structures.

Thus Meyer, like Weber after him, brought to the forefront the political focus of Yahwism and treated its struggles as resistance to alien forms of domination instead of simply to paganism and false gods. Furthermore, the struggles were not merely between one political form and another but between a typically Western (or Hellenic) form (individualist, independent, and culturally productive) and a typically Eastern form (statist, repressive, and ritualist). Meyer's characterization of these opposites was unabashedly value laden (Meyer [1884] 1939, pp. 207–18). (In his distaste for the ritualism and law of later Judaism, Meyer was in the good company of most contemporary Bible scholars, including Wellhausen.) But his commonplace value judgments did not obscure an unusual point of his analysis: many of the traits of later Judaism were attributable to the political needs of contemporary dominant political powers. This point emerged most dramatically in his treatment of the reconstruction of Jerusalem and the establishment of the Jewish congregation following the Babylonian exile (Meyer 1896). Chapters 4–7 of the Book of Ezra recount how the restoration of Jerusalem was carried out by returned exiles with the support of the kings of the new Persian empire (from Cyrus to Artaxerxes), support which resulted in the establishment of a theocratic rulership in Jerusalem. Meyer saw this as but a single instance of a general Persian policy of protection and support for local religious cults as a means to co-opting their priest-

hoods into the service of royal interests in their localities. For Meyer, therefore, the Jerusalemite theocracy was but a thinly veiled agency of Persian political control—a product of purely secular and completely external political interests.

Meyer's interpretation drew a passionately defensive rejoinder from Wellhausen, who, in support of his conviction of the essential integrity and independence of the course of development of Judaism, denied that Persian political need could account for the massive program of reconstruction carried through by the returned exiles in Jerusalem (Wellhausen 1897). Later evidence (especially from the Elephantine papyri [see Meyer 1912]) lent firm enough support to Meyer's argument so that Weber was completely convinced by it (Weber [1917–19] 1952, pp. 348–50, 427).

The general drift of Meyer's politician analysis of Israel's history and religion was important to Weber at a number of levels. First, it laid down the general terms of the questions to be addressed to Israelite history: the nature of Israel's peculiarity as a political and cultural transitional case between East and West, the formation of its religiosity out of the tensions in that transition, the manner in which imperial subjugation destroyed the context but preserved the content of that religiosity. Second, this set of questions reflected a basic approach to Israelite history that set it apart from the limitations of much religiously motivated scholarship: as against the "immanent evolutionism" of Wellhausian biblical scholars (the belief that Israelite religious development is determined by its "unique, intrinsic tendencies"), it preferred an "epigenetic" explanation, according to which Israelite religious development included accretions not essentially connected with its intrinsic tendencies or even necessarily originating from purely religious sources (Weber [1917–19] 1952, pp. 426–27; [1904] 1949, p. 128). Third, Meyer's analysis of the instance of epigenesis afforded by Persian intervention particularly struck Weber's fancy. For Meyer, the fate of Jerusalem (and of other similar religious centers in Asia Minor and Egypt) under Persian rule demonstrated the "objective possibility" that would have overtaken Athens and the Greek world had Persian arms conquered at Marathon and Persian influence swept down the Attic plain. The prospect of Persian mystery religion and Persian priestly domination infiltrating and transforming Greek culture thus established the Greek repulsion of Persia at Marathon as a key moment in the coming to life of the Hellenic world and "those cultural values from which we still draw our sustenance" (Weber [1904] 1949, pp. 171–72). Meyer's argument on this point struck Weber as "ingenious," not only because it highlighted the gulf that separated Greek culture from Persian and the crucial significance for world history of their continued separation, but also because it was an outstanding demonstration of the use of objective possibility as a quasi-experi-

mental device in the construction of historical hypotheses (Weber [1904] 1949, pp. 171–72, 174–77; 1978, pp. 11, 1160).

These readily apparent interconnections between Meyer's and Weber's work suggest the possibility of a wider influence of Meyer's writings on Weber than has yet been documented. On the other hand, some evident dissimilarities also exist. Weber did not share Meyer's fascination with Hellenism and, given his criticisms elsewhere of Meyer's overindulgence in drawing parallels between disparate historical situations (Weber [1909] 1976, pp. 43–44), did not pursue the issue of early Israel's cultural anticipation of Hellenism. Nor did Weber share Meyer's dismissive attitude toward later Judaism. More important, the elaboration of adequate concepts, which Weber considered the precondition for historical analysis, is nowhere nearly as highly developed in Meyer. Although Meyer had control over a vast range of empirical material on the ancient world, he did not attempt to develop the equivalent of Weber's *Economy and Society* as an essential part of his research project.¹² Thus his analyses lacked the background of conceptual clarification which guided Weber in relation to such central topics as priesthood, prophecy, ethical rationalization, and so on. The difference Weber's comparative and conceptual background makes is most evident in his unique perception of what is initially intriguing in the history of Judaism—the position of the Jews as a “caste in a casteless world.” The image, of course, is drawn from India, but the paradox of the Jews stands in contrast with Indian castes: for the Jews, ritualistic segregation is not supported from without by a total caste-based social order but is maintained entirely from within on the basis of a unique religious promise and at the cost of dislocation from the larger social world. Framed in these terms, the paradox of Judaism takes on an interest for its own sake and becomes, in sociological terms, something more than just a contributor to later world-historical developments.

However, despite the gaps between Weber and Meyer, the latter remains an interesting figure whose own work and whose contribution to Weber's intellectual development have only been touched on here. A proper intellectual history, not only of *Ancient Judaism*, but of Weber's work as a whole, would have to take closer account of the stimulus and the substantive ideas afforded by Meyer's writings.

¹² Meyer did preface volume 1 of his *Geschichte des Altertums* with a 250-page discussion of general ideas which he titled “Elements of Anthropology.” The latter third of this is a discussion of historical methodology with which Weber took issue elsewhere. The rest deals with the “general forms of human life and human development” in a loosely structured and evolutionistically tinged discussion which ranges over the social, the political, and most of all the cultural areas of human life in its ancient or primitive stages. It focuses on sets of oppositions (between external influence and internal motive, between tradition and progress, between the mass and the individual) that were the common stock of contemporary liberalism but bear little resemblance to Weber's more elaborate typologies.

CONCLUSION: *ANCIENT JUDAISM* AND HISTORICAL SOCIOLOGY

Weber's undoubted scholarly debt to researchers who preceded him in the field gives some substance to his own disclaimer of any intention to add anything essentially new to the discussion of the religion of Israel (Weber [1917-19] 1952, p. 425). Nevertheless, he had a concern to "emphasize some things differently than usual" which derived from his particular sociological background and the manner in which he applied it to historical problems. Thus, the peculiar questions of the pariah status of the Jews and of ethical rationalization as a process in Israelite religion are distinctively Weberian variations on previous approaches to the historical analysis of Judaism. However, if it was the conceptual reference to his sociological background (as reflected in *Economy and Society*) that gave Weber his distinctiveness among contemporary historians such as Meyer, it is the historical application of that conceptual reference that sets him apart from all but a minority of subsequent sociologists. Along with its substantive issues, *Ancient Judaism* is thus a useful indicator of the exercise of Weber's historical sociology in practical research. Dealing as it does with the development of a specific situation over time, it is the most historical of Weber's works in the conventional sense of the term. It is also comparative: because Israel was a small nation caught between and frequently penetrated by stronger neighboring civilizations, the elucidation of its structural and cultural individuality required a sustained exercise in comparative dissection to reveal the features and boundaries of its personality. The interpretative (*verstehendes*) element is employed with a subtlety equal to that of *The Protestant Ethic* and a firmer grounding in available evidence. It not only is applied to meaning complexes and meaning construction in social groups but also, in its analysis of the great prophets, deals with variations in the meaning systems of individual agents in history.¹⁸ It is thus a practical counterpart of Weber's discussions of individual meaning in the more abstract methodological sections of his work. Therefore, *Ancient Judaism* should be seen as an expression of a fully matured Weberian sociology at the peak of its substantive and methodological development. As an application of the formal constructs of *Economy and Society* in a concrete historical analysis, it denies the charge that Weber's "increasing formalism" in later life vitiated his historical perceptiveness (Anderson 1979, p. 410) or relegated historical research to the background of his concerns (Momm-

¹⁸ The analysis in *The Protestant Ethic* of the writings of such individuals as Richard Baxter and Benjamin Franklin does not have the same individual focus as the analysis of the prophets; it is instead a methodological device used to depict the meaning complexes of whole periods of European cultural development. With the prophets, in contrast, Weber showed an interest in personal motivation, psychic disposition, and all-round individual character (for want of a more exact term) that is rare in his sociology (for a shorter flash of a similar interest, see Weber's comments on Jesus [1978, pp. 630-34]).

sen 1974, pp. 4–5, 14). Indeed, it offers the clearest illustration of the combination of elements that distinguish his work: concept development, analysis of historical causal relationships, and interpretive analysis of meaning, all conducted with a concern for disentangling the strands of the entire course of Western civilization.

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The Arithmetic of Social Relations: The Interplay of Category and Network¹

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This paper develops a conceptual scheme that merges the qualitatively stated propositions of Blau's recent axiomatic theory of social structure with the quantitative approach of social network analysis. The conceptual scheme is used to describe a set of inescapable features of intergroup and intragroup relations. We examine, both qualitatively and in formal equations, the tautologies that govern contact rates and network densities for any population that can be divided into two categories. We show how assumptions about the partitioning of populations into social categories can be translated into precise probabilities of contact within and between categories. We present several illustrations of the immodest implications of apparently modest assumptions. Following simulations of a high school within an adult community and an old boy network within a larger bureaucracy, we apply our conceptual scheme to actual data on social relations within a regional elite. Accompanying these examples is a discussion of a new perspective on reference groups, as well as a development of a common exception to Blau's theory. We conclude with a formal statement of the substantive propositions that follow from our conceptual scheme.

When is a category a group? What is the relation of group and network? That there is some connection among these concepts is obvious. It is the goal of this essay to examine these interrelations within a quantitative conceptual scheme for intergroup relations.

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The issue might be framed in broadly Simmelian terms. Our concern is with the effect of number, including size and proportion, on social categories, networks, and groups. Number is ubiquitous and any conclusions or insights based on it have very broad potential for application. This essay will examine how a category becomes grouplike through the effect of category size and contact segregation on social networks. We will focus on the simple case of a dichotomy that may be identified with an ingroup/outgroup or a minority/majority distinction. Since nearly any group is a minority with respect to the rest of the world, this simple case is of very general import.

The core of our presentation is the implications of constraints (which appear as equations linking different quantities) that inhere in a numerical characterization of intergroup relations. Although the constraints are purely formal or arithmetical, striking social implications follow from intuitive interpretations of the sociological effects of quantity.

The numerical relationships hold true wherever the quantities dealt with exist. Even though the social implications are contingent and depend somewhat on the particularities of settings, the universal applicability of the numerical relationships lets them serve as an organizing schema for an array of comparisons and contrasts that would not otherwise be apparent.

The basis of the discussion is a simple quantitative picture of intergroup relations. One primitive term is category: categories exist when any scheme, standard, or distinction permits unique classification of persons. In the present paper we will limit ourselves to the simple dichotomous case (although polychotomous and multiple dimension generalizations that are quite similar may be found in Rytina [1980*a*, 1980*b*]). The other primitive term is dyadic contact. Like category it can take many forms, including marriage, friendship, and telephone calls. All that is necessary for present purposes is that the number of contacts observed is independent of which member of the dyad is observed. Thus such variables as time spent in interaction or number of words shared in conversation, which have the property of numerical symmetry, are amenable to the treatment explored in this paper. Numerical symmetry does not require social symmetry, as the perfectly suitable parent-child tie illustrates, or mutual acknowledgment. It only requires the existence, in principle, of a procedure that would yield identical quantities of contact or interaction when applied to either member of a dyad.

The result of assessing categories and contacts for a population may be summarized in a tie-accounts table, a double-entry accounting device illustrated in figure 1. In a two-category population, all contacts are either within the first category, within the second, or between the two categories. In addition to these quantities, presented as per capita rates, the tie-accounts table also includes the sizes of the two categories and the per capita

Origin of Ties	Number of Persons in Category	Destination of Ties		Average Number of Ties
		To group ₁	To group ₂	
From group ₁	N ₁	IN ₁ average number of ties from '1's to '1's	OUT ₁ average number of ties from '1's to '2's	T ₁
From group ₂	N ₂	OUT ₂ average number of ties from '2's to '1's	IN ₂ average number of ties from '2's to '2's	T ₂

Population Total = N

FIG. 1

tie totals for each. Such a table might summarize the aggregated results of a study of intercategory contacts, such as interracial friendships, and could indicate the condition of intercategory relations as a whole. But such tables, and the realities they summarize, are governed by constraints that often lead to surprising conclusions. Those constraints and their consequences are the topic of this essay.

Our concerns may be related to two strands of sociological thought. First, the concern with the effect and structure of dyadic contact is shared with the eclectic body of analysis and research on social networks.² The great promise of the network perspective is that macro and micro can be linked by examining the structural constraints imposed by relational configurations (Coleman 1958; White, Boorman, and Breiger 1976). But this has remained more promise than accomplishment because computational and conceptual complexity has limited applications to rather small collections of individuals. Our approach achieves a leap of scale by the simple if brutish strategy of aggregation. Instead of examining the detailed configuration of ties among individuals, our concern is the flows of contact, the totals and rates of ties within and between categories of individuals. The loss of detail is severe, but the gain in simplicity and scale is considerable. It allows us to suggest that some striking effects of network are likely to be present in larger populations than are usually considered.

The second related strand of theory is found in Blau's (1977a, 1977b)

² In fact, the first tie-accounts tables were constructed while trying to visualize the outcomes of applying Granovetter's (1976) network-sampling ideas to a study of race relations in a metropolitan area. In this historical sense, the present effort is directly descendant from the network tradition.

primitive theory of social structure. The underlying conceptual atoms are identical since Blau also examines disjoint collections of positions and numerically symmetric dyadic interaction. Blau's grand sweep also takes in continuous distributions of positions and multidimensional distributions to yield a theory that relates structural features with overall integration. Our goals are considerably more modest.⁸

Blau looked at the integration by intergroup relations of complex structures; we are more concerned with within-category configurations and the generic concepts of group and minority. Blau assimilated the notions of in-choosing category and social group; we will attempt to spell out the conditions of size, segregation, and contact frequency that make categories grouplike through their joint effects on the networks that surround individuals. Our analysis complements Blau's in another way. Formalization of the quantitative relations renders the implications of different numerical assumptions more exactly. Such precision should serve to sharpen appreciation of the impact of Blau's notions, although it also allows us to point out a large class of examples that run counter to Blau's axiom of in-category preference. By looking at groups rather than entire structures and by displaying the underlying formalism, we hope to point out a new realm of applications for this intersection of the network metaphor and Blau's numerical conception of social structure.

One feature of our analysis is somewhat awkward to defend. The formal relations that are at the core are "mere" tautologies and the relationships are true by definition. But tautology has received something of a bum rap. Schelling (1978) has provided many examples of insights from numerical tautologies that are similarly based on double-entry accounting properties of certain social symmetries, such as quantity bought and sold. He argues that definitional truth or pure logic will supply insight whenever the terms of the argument are not matters of everyday experience. He suggests the telling example of the nonobvious but tautological answers to algebraic story problems. Many of our results are similar. Rather modest assumptions can generate immodest structural contrasts. The contrasts are logically inherent in the assumptions, but the magnitude of the results is sufficiently striking to deserve careful attention. The results follow by pure logic, but they are not immediately obvious for all that.

If such a defense is awkward, the presentation is more so. The logical truths are quite simple. But conviction about the social implications requires some coaxing. In what follows there will be an interplay of purely formal presentation and argument by example to illustrate the sociological effects. In the first section below, "Mechanics 1," the basic logic will be presented by verbal and numerical example. Mathematically inclined

⁸ To recapitulate Blau's theory would take us far afield, but Turner's (1978) review provides a most accessible summary of its content.

readers may find this tedious; for them, "Mechanics 2" presents the argument as a set of formal manipulations. Those who do not enjoy wrestling with equations are invited to skip this second section. In a third section, various examples are used to illustrate the social implications of the formal results. The fourth section provides an empirical example for those disinclined to believe conclusions based on sheer speculation.⁴ This is followed by a summary of the substantive propositions that follow from the discussion and by a brief concluding section.

MECHANICS 1

The goal of this section is to demonstrate the interdependencies that govern entries in tie-accounts tables. For each category, such a table records category size, total per capita contacts, and the distribution of contacts among members and nonmembers. One issue is the effect of relative size on this distribution. A second issue is the extent to which the category boundary affects the contact distribution. Following Blau, we term the latter effect the "salience" of a category. A third issue is the relative grouplikeness of each category. As a first approximation, we will examine a measure of group cohesion based on the probability that members share a tie.

For a first example, imagine a population of blacks and whites. To fill out a tie-accounts table, three kinds of information are needed: (1) the sizes of the categories (in this example, let us assume 5,000 blacks and 95,000 whites); (2) the number of contacts made by an average population member (here assumed to be 500 acquaintances per member for both categories); and (3) the distribution of ties within and between categories. We start with a random distribution, such that tie allocation is unaffected by category boundaries, and then model segregation in later examples.

Each cell in figure 2 contains the average number of ties sent either within categories (IN_i , IN_j) or between categories (OUT_i , OUT_j). The lower right cell gives the average number of ties sent from a white to other whites. Since whites are 95% of the population, 95% of 500 or 475 is the cell entry when the boundary has no effect. The remaining 5% or 25 ties sent to blacks are entered in the lower left cell. (We could, equivalently, tally the total number of contacts for each cell, e.g., in the lower left cell [$95,000 \times 25 = 2,375,000$ ties], but we find more intuitive appeal in the average contact patterns for members in each category. Frank [1971] and

⁴ The empirical example may strike some as gratuitous since the definitional truth of the tautologies is hardly affected by whether real data or imaginary tables are manipulated. But some reviewers and other readers have indicated that actual data would increase their confidence in the results. And the empirical results do prove that some of the contingent assumptions employed in the discussion are true in at least the particular instance examined.

Origin of Ties	Number of Persons in Category	Destination of Ties		Average Number of Ties
		To Blacks	To Whites	
From Blacks	5,000	25	475	500
From Whites	95,000	25	475	500

Population Total 100,000

$d_{BB} = .005$ $d_{WW} = .005$

FIG. 2.—Tie-accounts table displaying random mix

Granovetter [1976] give general presentations of network flows in terms of average numbers of ties.)

The random mixing of ties illustrated in figure 2 constitutes an important special case, one in which social networks are completely unaffected by category memberships. This is a sort of null or baseline social structure, in the sense that most sociologically interesting categories produce patterns of interaction that depart from random mixing. We thus take random mixing to be the zero point for our measure of category salience, *I* (cf. Freeman's [1978] discussion of segregation in social networks). For group cohesion, random mixing means, by definition, that two members of the same category are no more likely to share a tie than any other two members of the population. This approach differs from Blau's, in that his assumption 1.1 states that two members of the same category should exhibit higher than random rates of in-choosing; the difference is not simply a matter of distinguishing between categories as abstract ideas and socially salient categories, for we will show that there is an important class of categorizations with negative salience, in other words, with lower than random rates of in-choosing.

For the moment, however, let us examine the more common case of positive salience, in which members of a given category do have some preference for interacting with similar others. Returning to our first example, we modify the random distribution of contact to reflect a segregation in which whites take 40% of the contacts sent previously to blacks and devote them

to other whites. The tie-accounts table for the population of figure 2 with the new distribution of ties is shown in figure 3. The process by which we move from figure 2 to the complete specification of figure 3 is instructive. Our new assumption about tie distribution actually affects only one of the four cells in the tie-accounts table: OUT_i in figure 3 must be 60% of OUT_i in figure 2. We can then calculate that IN_i in figure 3 is 485 because $IN_i + OUT_i = 500$. Further, we know that the average of 15 ties apiece sent from 95,000 whites must be met by an equal number of ties from the 5,000 blacks, yielding an average of 285 ties apiece for OUT_j .

What we have just seen is that the specification of one cell in the 2×2 tie-accounts table is sufficient to define the quantities in all the cells. Here we have the first example of the arithmetic identities underlying our tie-accounts tables. This is a general result based on the fact that there is only one degree of freedom in a two-category system of numerically symmetric interaction when the category sizes and the total number of ties per category are known. Thus, any assumption that determines a value for one cell in our tie-accounts table will in fact determine the entire table. For example, an assumption that 3 out of 1,000 of the possible ties from blacks to whites indeed exist (this assumption determines OUT_j) would define figure 3. So would the statement (determining IN_i) that whites send 97% of their ties to members of their own group, or the assumption (determining IN_j) that blacks would have to decrease their rate of in-choosing by 88% to achieve random mixing. Similarly, we could have noted that blacks are 8.4 times as likely to share a tie as whites and reasoned back to any and all

Origin of Ties	Number of Persons in Category	Destination of Ties		Average Number of Ties
		To Blacks	To Whites	
From Blacks	5,000	215	285	500
From Whites	95,000	15	485	500

Population Total 100,000

$d_{BB} = .0430$ $d_{WW} = .0051$

FIG. 3.—Tie-accounts table displaying in-choosing

of these assumptions. The fact that each of these assumptions defines the same tie-accounts table means not only that they are equivalent but also that to make any one of them is, by definition, to have made them all. Our tie-accounts table is the schematic representation of a system of tautologies which allows us to assign any given assumption to a large set of mutually implied assumptions.

As we have noted, we will want substantive indices that summarize our tie-accounts tables. The first index, our measure of category salience, I , which increased from 0.0 to .40, corresponds to the initial assumption of segregation that we used to define figure 3: the percentage decrease in out-group interaction in comparison with random mixing. This index ranges from 0.0 or no category salience (under random mixing), to 1.0 or complete salience (with all ties sent within categories); it assumes a negative value when more ties are sent to out groups than would have occurred under random mixing.

A second index, our measure of group cohesion, will be based on the probability that two randomly selected members of a category share a tie. Formally, this is the network density, or the proportion of all possible ties that in fact exist (shown as d_{ii} , etc., for the specific cells, and D for the total population). Under random mixing, the probabilities for contact across the category boundary or within either category are the same as the probability of contact in the population as a whole (.005 in fig. 2). When category salience is positive, ties will be transferred from the two OUT cells to the corresponding IN cells, creating higher densities within categories than in the total population. But, as the 8.4 ratio between d_{jj} and d_{ii} in figure 3 shows, the probability of within-category contact will increase more rapidly in the minority than in the majority. In this example, every unit decrease in OUT_i corresponds to 19 fewer ties for OUT_i and produces increases in d_{ii} and d_{jj} of $1/95,000$ (or .00001) and $19/5,000$ (or .0038), respectively. (Note that this result, and thus the 8.4 ratio, holds independently of the assumption of 500 or any other number of ties for both categories.)

These differences in changes in density occur in any categorization that pairs a numerically large majority with a small minority. It is reminiscent of adjusting a teeter-totter to balance two very different weights: the "heavier" majority will be very near the center, while the small minority will be a great distance from the fulcrum; even slight changes in the "weight" of the majority will produce extreme shifts in the position of the minority. Both out-contact rates and their complement, cohesion, rotate around the fulcrum. With this analogy in mind, we use the term "leverage" to refer to the magnified effect that differences in tie distributions have on the cohesion and out-contact rates of numerically smaller minorities.

To complete this nontechnical summary, we wish to relax one of the

assumptions made to simplify our presentation so far: that both categories have the same number of ties per member. In figure 4, whites distribute their ties as in figure 3, but we have increased their ties per person by 200. Several points can be made. First, there is still only one degree of freedom in the table. That result requires only that we know the average total number of contacts for members of each category (T_i and T_j). Second, the difference in numbers of contacts leads each category to have its own salience. Because whites distribute their contacts as in figure 3, the salience of their category does not change, but because blacks devote proportionately fewer of their ties to their own category, it has a lower salience. Third, once again we see forms of leverage in our cohesion (or density) indices for the two groups. The small increase in the majority's group cohesion is matched by a much larger decrease in cohesion for the minority. Finally, the extent to which we have lowered IN_j should warn us that there are some sets of assumptions that cannot be made. If we had doubled the number of ties per white to 1,000 without changing our assumptions about how ties were distributed; OUT_j would be 570, which is more than the total number of ties we have allotted to blacks. When real data are organized into a tie-accounts table, such inconsistencies can never occur; but, if the table is being used to model a set of substantively motivated assumptions, analysts must take it upon themselves to avoid assuming that $2 + 2 = 5$.

Origin of Ties	Number of Persons in Category	Destination of Ties		Average Number of Ties
		To Blacks	To Whites	
From Blacks	5,000	101	399	500
From Whites	95,000	21	679	700

Population Total 100,000

$$d_{BB} = .0202$$

$$d_{WW} = .00714$$

$$I_B = .16$$

$$I_W = .4$$

FIG. 4.—Tie-accounts table displaying unequal tie totals

MECHANICS 2

Assume that a population of size N can be classified into two categories of sizes n_1 and n_2 , where $n_1 + n_2 = N$. Assume that an average member of category 1 has T_1 contacts, and that these are allocated as IN_1 (average contacts from members of category 1 to each other) and OUT_1 (average contacts from members of category 1 to category 2), with $IN_1 + OUT_1 = T_1$. A corresponding statement may be made about category 2, with the average member assumed to have T_2 contacts.

If contacts are numerically symmetric and the totals at origin and destination are identical, then

$$n_1 * OUT_1 = n_2 * OUT_2, \quad (1)$$

or

$$n_1/n_2 = OUT_2/OUT_1, \quad (2)$$

so that the rates of out contact are in inverse proportion to the relative sizes of categories.

If n_1 , n_2 , T_1 , and T_2 are known, then a knowledge of any one of IN_1 , OUT_1 , IN_2 , and OUT_2 is sufficient to determine the other three. Thus there is only one degree of freedom for the configuration.

Let I be an index of salience while k ($= 1 - I$), its additive complement, is an index of segregation. Define k_i as

$$k_i = \frac{OUT_i/T_i}{n_j/N}, \quad (3)$$

or equivalently

$$k_i = \left(1 - \frac{IN_i}{T_i}\right) / \left(1 - \frac{n_i}{N}\right), \quad (4)$$

so that knowledge of either IN_i or OUT_i suffices to determine I or k .

Next consider the relation between the configurations of the categories. From equation (1) note that

$$OUT_1 = \frac{n_2 * OUT_2}{n_1}. \quad (5)$$

Substituting equation (5) into equation (3) yields

$$k_1 = \frac{n_2 * OUT_2 / n_1 * T_1}{n_2/N} = \frac{OUT_2/T_1}{n_1/N} = \frac{T_2 k_2}{T_1}. \quad (6)$$

This substitution reveals that, first, the k 's are in inverse proportion to the tie totals so that the better endowed category has higher salience (smaller k) and, second, the saliences are equal in the special case of equal tie totals.

Equation (5) also reveals a first sort of leverage. Since the out contact totals are in proportion to inverse sizes, any change in the total of the larger category corresponds to a proportionately greater change in the total of the smaller category.

As noted in the previous section, the salience index is directly related to the cohesion measure, density. Density is defined as

$$d_{ii} = \frac{n_i I n_i}{n_i(n_i - 1)} \simeq \frac{I n_i}{n_i}, \quad (7a)$$

$$D = \frac{NT}{N(N - 1)} \simeq \frac{T}{N}, \quad (7b)$$

where d_{ii} is the within-category density and D is the population density. Unless categories are quite small, the approximations make a negligible difference.

To get a readable expression describing the relation of size and salience, we assume the simplification that $T_1 = T_2 = T$, substitute $P_i = n_i/N$ and $I + (1 - I)P_i$ for $I n_i$, and calculate the relative density

$$\frac{d_{ii}}{D} = \frac{I}{P_i} + 1 - I, \quad (8)$$

which describes the extent to which the within-category density exceeds the population or random mix density. Figure 5 displays the relationship of relative density and proportional size for several different values of salience.

Either figure 5 or inspection of equation (8) yields three generalizations. When salience is positive, relative density increases as relative size decreases. The rate of increase is greater when salience is greater. Conversely,

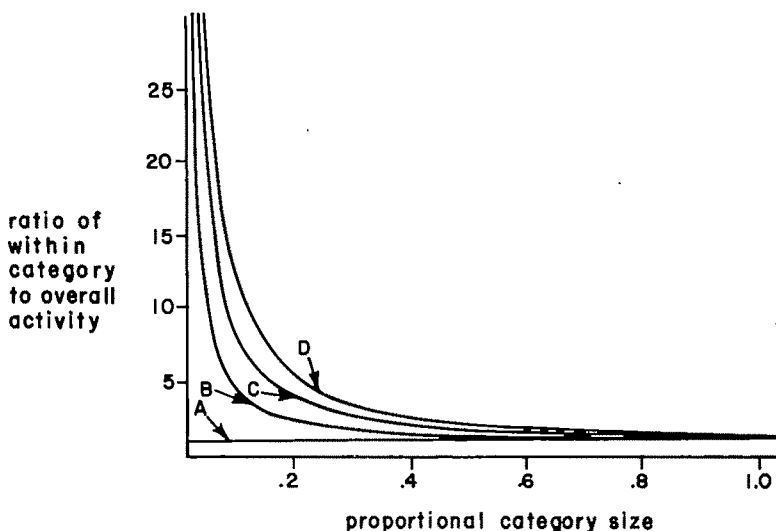


FIG. 5.—Ratio of within-category density to overall density as a function of proportional category size and salience. The lines A, B, C, and D refer to saliences (I) of 0.0, .33, .67, and 1.0, respectively.

the leverage effect is that the result of a change in salience is in inverse proportion to category size.

SUBSTANTIVE APPLICATIONS

In this section we will demonstrate how to convert the information in tie-accounts tables into substantive interpretations. Our strategy for representing and interpreting network flows in social settings begins with a numerical simulation of the given setting and then proceeds to a series of inferences about what is promoted or prohibited within this simulation. This includes, as we shall see, the possibility of "testing" alternative interpretations via appropriate modifications of our assumptions.

Consider a high school with 2,000 students, serving a community in which the total population over age 15 is 50,000. While there is undoubtedly some tendency for Americans to concentrate their social contacts among age peers throughout the life cycle, we believe that the nature of high schools as institutions heightens this separation for adolescents. The social density among students is increased both by their segregation from the adult community and by their self-selection based on specialized activities. In figure 6, we have assigned the usual 500 ties per person to adults but only 300 to students because they are younger. An assumption of $I = .65$ for students completes the definition of the table, that is, they withhold about two-thirds of the ties that would have gone to adults under random mixing. (Note that

Origin of Ties	Number of Persons in Category	Destination of Ties		Average Number of Ties
		To Students	To Adults	
From Students	2,000	200	100	300
From Adults	48,000	4.17	495.83	500

Population Total 50,000

$$d_{SS} = .1 \quad d_{AA} = .01$$

$$I_S = .65 \quad I_A = .79$$

FIG. 6.—Tie-accounts table displaying adolescent-adult relations

because of the difference in tie volumes, adults actually have a higher category salience; see eq. [6].)⁵

Students, in this simulation, activate one in ten of 2,000,000 possible ties ($2,000 \times 2,000/2$), a rate which is almost 10 times higher than the density for adults. If anything, we underestimate within-category contacts of the average student, for we have distributed ties randomly within this group, while high schools are typically differentiated according to one's year in school and, within year, by program of studies. Just as segregation and self-selection pushes the social density of students to .1, well above the value of .01 for the total community, so will selection within the school further increase densities there. Thus if our simulation included a division between 600 seniors and 1,400 juniors and sophomores, with seniors having a salience of .67, the internal density for the latter would be .20, twice that of the school as a whole.

The result is something with which we are all familiar: individuals attached to peer groups and peer groups connected into a distinctive segment of the community: the so-called adolescent society (Coleman 1961). Here, the result is generated by social and institutional factors involving age grading, not by unavoidable aspects of adolescents' psychosocial development, etc.

Let us consider some general inferences about situations such as that modeled in figure 6, beginning with the widespread visibility of persons and their activities under conditions of high density. On the one hand, density may create disadvantages due to lack of privacy, especially when privacy is conceived of as maintaining the separation among role performances before different audiences. On the other hand, when access to a variety of informants and their information is desirable, there are certain advantages to high density. It means that an individual's contacts are likely to penetrate a relatively diverse set of social circles, and the range of information which is potentially available is quite large. The fact that this situation differs sharply from the experience of those in the surrounding community can lead to serious misperceptions by the majority. For example, parents might perceive a variety of troubling activities to be statistically more common (if not socially more acceptable) among adolescents than among adults. In reality, if such activities were no more common,

⁵ Anybody looking at such a table might take issue with some of its entries as unrealistic. One reader felt that the 4.17 contacts from the average adult was unrealistically low. First, it should be noted that such a number is an average. Boy Scout leaders and guidance counselors know more adolescents, but the cell entry depicts the average for all adults. Second, and more important in this context, the cell entry is a function of other entries in the table. If 4.17 is unrealistically low, so is the 100 that describes student-to-adult contacts since the two entries summarize exactly the same reality. A more realistic 10 adult-to-student contacts implies a less realistic 240 student-to-adult contacts. One point of the present exposition is that the intuition that sees 4.17 as low but 100 as high is not sufficiently guided by the numerical logic of the situation.

they would still be more visible. The adolescents are more likely to hear about and have contact with activities that the average adult can unthinkingly avoid. Even "morally sound" adolescents will have more awareness of the rationales, occasions, and likely participants for deviant routines than their adult counterparts.

A related set of interpretations concerns the difficulty of maintaining social differentiation: when density is high, large differences in social background, preferred activities, etc., are not as readily translated into small probabilities for contact. In our example, two randomly selected students are no more likely to be socially similar than any two randomly selected adults, yet the two students are far more likely to be in contact.⁶ Further, categories which combine relatively small size and high density interfere with the use of selectivity to control one's range of contacts: an adult in the simulated community of figure 6 can reject 99% of his fellow adults and still have 480 potential adult acquaintances, while an equally selective student has only 20 potential acquaintances among students. This threat of relative isolation may be one component of peer pressure among adolescents. Not only are violations of values and standards more visible, but the violator may be both cut off from valuable sources of information and forced to limit associations to a smaller subset of the student minority.

Instead of pursuing the content of tie-accounts tables for high schools (which is after all an empirical matter), we wish to point out some of the more general features of this substantive example. In particular, we wish to return to one of the issues used to introduce this paper: the ways in which groups correspond to an overlap between categories and networks. Readers well versed in Merton's (1968) theory of reference groups may already have recognized the redevelopment of some of the central tenets of that theory in our substantive interpretations. We believe that categories which are sufficiently salient and sufficiently dense, such as the students in this example, contain many of the features of groups that are associated with the notion of reference group.

Merton's minimal defining characteristic for membership in a group was that interaction with group members must be more common than with nonmembers, that is, category salience must be greater than zero. As strengthening conditions, he added, members must be defined as such by themselves and by outsiders. Either our arithmetic or Blau's axiomatics can be used, however, to show that these should not be thought of as independent criteria, for the existence of a highly salient category means both

⁶ Strictly speaking, the identical dissimilarity of random student and adult pairs applies only to family social characteristics. Obviously excluded are such characteristics as age. But included are such things as religion, family social status, and all the myriad differences in manners and mores for which the more general terms stand proxy. And the argument extends to any variable. The proportion of total subpopulation variety included in an average contact net will be far greater in the minority population.

a literal separation of its members from the larger population and a greater likelihood of connection between members.

The greater likelihood of connection is equivalent to the condition of mutual visibility that Merton thought crucial for the enforcement of distinctive norms and practices. While it is shared orientation to such norms and practices that distinguishes a group from a mere aggregate, the foregoing analysis shows that the mutual visibility that buttresses a shared orientation is contingent on category size, salience, and amount of contact. The structural properties are independent of the specific contents or meanings of membership; therefore the structural analysis is of general applicability. Potential reference groups vary over a wide range in these structural properties, and it follows that their effects on individuals should vary in a parallel fashion.

If Merton is correct in his attribution of importance to visibility, the potency of reference group phenomena should depend on category size. The number of contacts and degree of segregation have effects of comparable magnitude. Relatively large, highly segregated, and gregarious aggregates should be comparable with smaller, less segregated, and less gregarious aggregates. And the effects operate across a very wide range of scales. Quite small minorities, such as Kanter's tokens (1977*a*, 1977*b*), can combine substantial out contact with near total saturation within the category. Much larger minorities, such as residents of ghettos and urban villages (Gans 1962; Liebow 1967), can be substantially segregated yet lack sufficiently extensive contacts to unify the group.

It is useful analytically to separate segregation and saturation. When size is constant, the contrast arises as a result of differences in contact frequency. If individual isolation parallels aggregate isolation, a segregated but unsaturated aggregate is the result. But a gregarious aggregate may be saturated without necessarily being highly segregated. Membership in a segregated aggregate means that role performances are played out mainly in front of other category members. Standards that are uniform across the category, such as longstanding cultural traditions, will be strictly enforceable. Membership in a saturated aggregate means that performances are visible to all other members even if nonmembers figure prominently. Even innovative or newly fashionable standards can be enforced. One might expect the sharpest difference under the highly demanding conditions of conflict. A segregated aggregate can enforce traditional responses, while a saturated group can respond flexibly to the exigencies of the particular struggle. Gregarious aggregates can be saturated without being completely segregated and can maintain a given degree of saturation with a larger size so that their conflict capacity will be greater.

The mutual visibility within a saturated aggregate facilitates an informal solution to Olson's (1965) problem of the "free rider" since sanctions of

shame and esteem can be more effectively employed. And if such authors as Brecher (1972), Oberschall (1973), and Piven and Cloward (1977) are correct about the inhibiting effects of formal organization, the informal solution is conducive to radical or violent strategies. In short, a gregarious aggregate is a potentially angry actor.

Not all aggregates are aggrieved and not all reference groups are membership groups. Another important type is the nonmembership group whose standards are emulated by others. In Merton's analysis, emulation rests on aspirations toward membership as captured in the concept of anticipatory socialization. But this useful notion is too limited. Emulation without reasonable hope of acceptance is not only imaginable but quite common. In the next example, we shall consider a configuration of contacts conducive to style setting by a minority.

The obvious explanation of style setting by a minority is the possession by the minority of greater prestige or resources than are possessed by those who conform without being accepted into the minority. Our task is to indicate how a contact configuration that is probably quite common can point toward the same result. Although prestige and resources would no doubt help, our structural variable is the quantity of connections.

The setting is a government bureaucracy with 10,000 employees. Most of them were drawn to their present positions from diverse geographic and social origins. But a minority of 1,000 are drawn from elite backgrounds of exclusive suburbs, private schools, Ivy League colleges, and a highly selective set of postgraduate educational centers. In effect, such individuals were on the track toward their ultimate career destination at an earlier stage in the life cycle than others drawn from more diverse origins. As a result, more of their contacts are carried over from past experience into that destination. Such contacts not only are more numerous; they also have more of the depth that comes with longevity. But the first assumption, numerical abundance, is the important one. To make it concrete, suppose that each of the "old boys" has contact with 250 other old boys. The group of more diverse origins has shared less life history with other members of the bureaucracy so that only 100 ties to others in their category are assumed (see fig. 7).

This is insufficient to determine a tie-accounts configuration without a further assumption about the quantities in the between-category cells. But the given information does determine the density for the two categories. The rather modest difference in contact rates of 2.5 to 1 implies an immodest difference in densities of 22.5 to 1. (This is the product of the ratio of ties and the ratio of sizes.)

Another difference is even more striking. If two randomly selected old boys examine their acquaintance circles, the expected number of mutual others (EMO), or shared acquaintances, is 62.5. That is, each old boy

Origin of Ties	Number of Persons in Category	Destination of Ties To		Average Number of Ties
		Old Boys	Arrivistes	
From Old Boys	1,000	250	100	350
From Arrivistes	9,000	11.1	100	111.1
Population Total 10,000				
$d_{OO} = .25$ $d_{NN} = .014$				
$I_O = .68$ $I_N = 0.0$				
$EMO_{OO} = 62.5$ $EMO_{NN} = 1.11$				

FIG. 7.—Tie-accounts table for old boys in bureaucracy displaying modest out-group demand.

has contact with 25% of each other old boy's 250 contacts within his own group. For the other bureaucrats, this expected mutuality for a random contact is only 1.11 shared acquaintances. This large difference is entailed by the original assumptions and in a strict sense gives no additional information. But this more striking expression of the original assumptions highlights the contrast in the conditions of the two categories.

A major implication of these differences is that the quality and quantity of information available to old boys about each other is vastly greater than that available to the others. A further implication is that old boys are in a far better position to make arrangements with each other on the basis of mutual trust and to enforce obligations and reciprocity within their circle. Access to the collective history makes it easier to anticipate reliability and to avoid the untrustworthy. As a result, such a densely interconnected group is a suitable arena for the secure extension of social credit and, resource differences aside, this greater liquidity makes old boys better partners in bureaucratic barter. More extensive knowledge of opportunities and intentions would facilitate the formation of larger, more effective coalitions.

Since no actual bureaucracy would function with complete segregation between different social segments, the implications of contacts between the groups must be examined. Old boys' access to coalitions makes them highly

desirable interaction partners. But rather modest additions of arriviste demand have a dramatic effect. One contact per arriviste translates through leverage into nine new contacts per old boy; 11.1 contacts per arriviste translates into 100 contacts per old boy. This configuration is illustrated in figure 7. The relative densities are as stated above, but a great contrast in salience is apparent. Arrivistes have a random mix salience of 0.0 while old boys have the much higher salience of .68. Further modest additions to arriviste contacts produce the configuration of figure 8, where arrivistes are assigned 20 intergroup contacts. At that rate they are overchoosing old boys, and the salience for arrivistes is $-.67$. The old boys retain a positive salience of .53. They are highly connected and mutually visible, and yet each has more contact with the majority group than majority group members have with each other. Thus the minority combines greater cohesion and positive selectivity with an abundance of out-group ties. The majority has negligible cohesion and negative selectivity; only their out-group ties connect them to an arena of mutual visibility. And only the old boys, with their twin advantages of contact frequency and a small group, can maintain such an arena.

Other dynamic paths generate the same cross-sectional configuration. For example, in voluntary associations, including political organizations, the number of contacts presumably increases with time spent in the service

Origin of Ties From	Number of Persons in Category	Destination of Ties To		Average Number of Ties
		Old Boys	Arrivistes	
Old Boys	1,000	250	180	430
Arrivistes	9,000	20	100	120

Population Total 10,000

$$d_{OO} = .25 \quad d_{NN} = .011$$

$$I_{NN} = .53 \quad I = -.67$$

FIG. 8.—Tie-accounts table for old boys in bureaucracy depicting negative salience

of the association. It probably also increases with desirability as an interaction partner which, in turn, increases with social status. Thus higher status members and more active members are likely to have an abundance of contacts relative to lower status and less active members. That higher status members are more likely to be more active renders the configuration more likely. If such people collectively combine the size of a minority, modest self-selection, and an abundance of contacts, the result is a connected core and a disconnected collection of hangers on.

As in the previous example, the core group enjoys the advantages of mutual visibility. Its members' advantage in contacts and the higher quality of their contacts (with others having parallel advantages) result in a dominant position with respect to insider information. If the desirability of core contacts is great enough, the typical insider even knows more hangers on than does the typical member of the less active majority. Therefore insider information is more readily disseminated. That differential interests of insiders are more readily realized is a plausible conclusion. And that this configuration corresponds to many social arenas where rates of participation vary sharply is logically inescapable.

Such an example does not prove that insiders always dominate since it rests on assumptions that, though plausible, are obviously not general. But such a demonstration was not the purpose. All we wished to show was that modest assumptions about size, contacts, and in-choice can imply striking contrasts when the implications of aggregation into categories are considered. In the next section we will use data on an elite network to show that many of our assumptions about size, contact rates, and in-choices are reasonable. And the presentation of the data will provide a further illustration of the numerical transformations of the previous discussion.

EMPIRICAL APPLICATIONS

Any empirical applications of our techniques would require estimates of the two groups' sizes and the four contact rates, T_u , T_y , T_μ , and T_ν . The means of data collection must depend largely on the number of ties in the network. For either small populations with many ties per person (e.g., a high school) or large populations with few ties (e.g., ethnic intermarriage), a complete enumeration of the network is feasible. When both the size of the population and the number of ties per person are large, some form of sampling is necessary. Sampling techniques involving surveys (Granovetter 1976) have been described but not systematically tested.

Our own examples utilize data on 200 members of the northeastern metropolitan elite. These data were originally collected in 1969 by Delbert Miller, who used a modified reputational approach (Miller 1975). Each

respondent was given a list of the 200 members of the sample and was questioned about the range of contacts he or she had had with each of the others. This provides us with the data to estimate contact rates within and between various subsets of the elite. The survey placed a special emphasis on the position of black leaders in the region, and a substantial proportion (18%) of the respondents were black. This provides us with our first application: an opportunity to examine the aggregated networks of a potentially dense minority within a larger population. For an empirical illustration of our work on the network configuration of elites, we investigate a categorization of the same sample which distinguishes the best known respondents from the others.

In order to convert the raw data to tie-accounts tables, we must first confront the problem of symmetry, that is, our assumption that T_{ij} and T_{ji} both imply the same total number of cross-category contacts. In the present case, we have calculated the total number of ties implied by both of the contact rates and used their average as the basis for estimating the amount of cross-category contact. In sociometric terms, this can be shown to be equivalent to an assumption that one-half of all the asymmetrically claimed ties are present.⁷ This technique assumes that the rates derived from either category are equally reliable. In the absence of better evidence, we believe that this is the safest assumption; if, however, data are collected with predetermined categories in mind, the analyst would do well to build in checks for differential reliability, for example, upward bias in friendship choices.

A final consideration, which is specific to this data set, concerns the problem of missing data. As is not uncommon with large-scale surveys of elites, this study was subject to a 50% nonresponse rate. This means that 100 members of the elite have given us data about their contacts with the full set of 200. We thus have two options for constructing our estimates of the contact rates: we can use either the 100×100 matrix of ties among respondents to the survey or the 100×200 matrix of ties sent (Miller used the latter). We have in fact constructed tie-accounts tables corresponding to both of these approaches, and we find that using the more restrictive data set does not meaningfully alter the conclusions reached by using the full data available to us. Because the larger data set is more directly comparable to Miller's original work, we will proceed with the estimates based on a N of 200.

Figures 9 and 10 show the exchange of ties between blacks and whites for the two relations that were most likely to produce symmetric contact: joint membership on committees and close personal friendship. In our

⁷ Traditional sociometric techniques of symmetrizing data treat all asymmetric ties as either completely present or completely absent. Because we are dealing only with rates of contact, we do not have to decide which ties are present or absent, so we may define any proportion of the asymmetric ties as present.

Origin of Ties	Number of Persons in Category	Destination of Ties		Average Number of Ties
		To Blacks	To Whites	
From Blacks	36	8.22	16.8	25.02
From Whites	164	3.69	17.41	21.10

Population Total 200

$$d_{BB} = .235$$

$$d_{WW} = .107$$

$$I_B = .18$$

$$I_W = .02$$

FIG. 9.—Tie-accounts table depicting racial distinction in elite sample for the tie "served on a committee with."

Origin of Ties	Number of Persons in Category	Destination of Ties		Average Number of Ties
		To Blacks	To Whites	
From Blacks	36	6.50	7.50	14.00
From Whites	164	1.55	8.27	9.82

Population Total 200

$$d_{BB} = .18$$

$$d_{WW} = .05$$

$$I_B = .35$$

$$I_W = .12$$

FIG. 10.—Tie-accounts table depicting racial distinction in elite sample for the tie "close personal friend."

earlier discussion of category inbreeding, we noted that even when T_{WW} was higher than T_{BB} , the minority would still have a higher density if the size differential were large enough. The minority is indeed denser for both types of ties examined here. In fact, two randomly selected black members of the elite are twice as likely to serve on the same committee as are two randomly selected whites. Close personal friendships are more than three times more probable between pairs of blacks than between whites. An important factor here is that blacks have more ties per person (approximately 25 vs. 21 for committees and 14 vs. 10 for friendships). This leads the two categories to exhibit different saliences (see eq. [7]). Surprisingly enough, the whites' salience of .02 (fig. 9) and .12 (fig. 10) indicate that they distribute their ties in a nearly random manner. Blacks, on the other hand, use their advantage in number of ties to overchoose their own group, resulting in both the higher density and higher category salience proposed in our earlier model.

Substantively, we may ask why black members of this elite seem to pay more attention to the category boundary. One strong possibility is the sub-cultural nature of this segment of the elite: they have and have had their own committees and constituencies. It is thus quite likely that the increasing number of ties sent by whites to black leaders in the late sixties came in addition to an already well-developed network within the black elite. The higher tie volume for black leaders lends some support to this hypothesis. If this were true, it would be of some interest to see whether black leaders have been able to sustain the higher level of activity necessary for any minority to be both internally dense and highly connected to the majority!

It will be recalled that just such a higher level of activity was one of the hallmarks of our old boy model. In that model, the upper elite was active enough not only to show high internal density but also to absorb so many cross-boundary contacts that the lower category exhibited negative salience. As shown in figures 11 and 12, this is precisely what happens for both committee memberships and close friendships when we split the elite at the median with regard to how often each member has been heard of by other members of the elite. Even this conservative definition of the upper elite shows all the characteristics of our old boy model.⁸ Members of the upper elite are also more likely to share either of the relations with each other. Indeed, the lower elite send ties to the upper elite at a higher rate than they send ties to their own category—in violation of Blau's axiom of positive in-choice.

⁸ Note that our upper elite differs from the smaller "top of the top" category used by Miller. We explored several techniques for defining a more exclusive set of upper elites but did not pursue them because of severe missing-data problems.

Origin of Ties	Number of Persons in Category	Destination of Ties		Average Number of Ties
		To Uppers	To Lowers	
From Uppers	102	19.16	9.96	29.12
From Lowers	98	10.37	6.44	16.81
Population Total 200				
		$d_{UU} = .19$	$d_{LL} = .066$	
		$I_U = .30$	$I_L = -.21$	

FIG. 11.—Tie-accounts table depicting status distinction in elite sample for the tie "served on a committee with."

Origin of Ties	Number of Persons in Category	Destination of Ties		Average Number of Ties
		To Uppers	To Lowers	
From Uppers	102	9.37	4.70	14.07
From Lowers	98	4.88	3.52	8.40
Population Total 200				
		$d_{UU} = .0928$	$d_{LL} = .036$	
		$I_U = .32$	$I_L = -.14$	

FIG. 12.—Tie-accounts table depicting status distinction in elite sample for the tie "close personal friend."

SUMMARY OF SUBSTANTIVE PROPOSITIONS

The smaller and more segregated a category is, and the more numerous its within-group contacts, the greater is its sociometric density. The greater the density, the harder it is for subgroups to maintain social isolation or privacy. As a result, any activities within the group, including deviant activities, will be more widely known and accessible to other group members.

The greater the density of a category, the greater the mutual visibility of its members. Therefore violations of group standards are more easily known and informal sanctions are more easily applied. Rewards for upholding group standards are more certain. Thus the extent to which a category has the impact of a reference group depends on size, segregation, and number of contacts.

The more gregarious an aggregate's members, the more size and saturation can be combined. Since this pattern facilitates informal coordination of contributions to collective goals, and since informal coordination is less temperate than formal coordination, a gregarious aggregate is a potentially angry actor.

If some participants in a setting have a longer shared history, in feeder institutions or in the setting itself, they will have more contacts. If they are in the minority, they will have far greater cohesion. To the extent that mutuality enhances bargaining, they will be in greater demand as contacts. The result is a minority that combines vastly greater internal cohesion with extensiveness of contacts with the majority. Access to an arena where trustworthiness is readily enforced, together with breadth of contacts, facilitates minority domination in such a setting.

CONCLUSIONS

Although the empirical example presented above used conventional network data, we hope to encourage another realm of applications for the ideas developed in this paper. Simple assumptions about size, contacts, and in-choice rates can imply striking differences when the implications of aggregation into categories are considered. The aggregation principle is a tautology, but its application can suggest new interpretations by depicting the same phenomenon from a different point of view.

One major theme is that given divergences in individual level configurations may look quite different from the group point of view. A second is that the conditions of two groups united in interaction are interdependent and that to hypothesize about one group while ignoring its complement is to comprehend only part of the hypothesis. To hypothesize about each independently is to risk logical inconsistency. With experience, the different viewpoints of individual and group or minority and majority are rec-

ognized as a single whole, but the shift in angle of vision can stimulate the imagination.

A still grander shift in viewpoint is provided by Blau, with whom we share a concern for size, contact rates, and in-choices. The exposition above reveals the underlying quantitative relations that are at the heart of his primitive theory of social structure. We did not follow his lead into macro theorizing but instead attempted to demonstrate the usefulness of these notions in a more modest concern for less complex social settings of the middle range. Although we relaxed his assumption of equal contact rates and provided examples of categories that display negative salience, we do not disagree with his adoption of these reasonable and highly powerful simplifying assumptions for his larger enterprise.

But our hope is to encourage another line of applications. The concepts of size, contact rates, and even in-choice can often be guesstimated or even estimated. Consideration of implications at the design or interpretation stage of research can suggest new questions and different insights. The simple relationships presented here are only a starting point and will hardly overwhelm other substantive considerations. But they are simple, they are very broadly applicable, and they can suggest social facts worthy of attention.

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Using Association Models in Sociological Research: Some Examples¹

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This paper discusses the log-multiplicative association model and demonstrates how it can be used in a variety of research situations in which ordinal variables are encountered. The examples pertain to (a) reconciling response distributions that differ because of question wording (or other context effects), (b) assigning a metric to an ordinal variable, (c) assigning scale scores to response patterns which arise from a Guttman-type model, and (d) analyzing multiple (ordinal) indicators. The log-multiplicative model presents a unified framework for analyzing each of these problems.

This paper considers some suggestive applications of the log-multiplicative association model recently presented by Goodman (1979; also see Andersen 1980). This model does not appear to have been utilized thus far in social research, even though a similar log-linear model has been used rather frequently (see Goodman 1981c). However, the log-multiplicative model has a rich potential for sociological application, and the objective of this paper is to present examples which suggest some uses the model might ultimately have.

The chief advantage of the log-multiplicative model, at least for the examples considered here, is that it provides information about intervals between categories of ordinal variables. Each example considered stresses this aspect of the model, and the assumptions necessary to "scale" ordinal variables are explicitly stated and critically discussed. First I consider how response distributions obtained from two similar questions on attitudes toward the courts can be reconciled by appealing to the association model. An interesting feature of this application consists of discerning the proper placement of the response, don't know, along the underlying scale of the attitude. The second example pertains to the "general happiness" item used

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in the General Social Survey, as well as in other surveys. This item elicits one of three possible ordered responses (not too happy, pretty happy, very happy); it is a simple example of an ordinal variable. By means of a certain conditional association model, it is estimated that the actual distance between the first two categories ("not too happy" and "pretty happy") is three times the distance between the second two categories ("pretty happy" and "very happy"). The third example deals with three items on the attitude toward legal abortion. By assuming that a Guttman-type model underlies the observed responses to these items, both the scale-response patterns and the error-response patterns are assigned scale scores. These scale scores could be used to construct an interval-level variable for use in causal models involving the attitude on abortion. The final example pertains to a cross-classification of three ordinal indicators of satisfaction with life, data that would be analyzed conventionally with the common-factor model. The partial association model applied to these data shows conclusively that (a) the assumption of equal intervals can be rejected, (b) the joint distribution of the variables cannot be the trivariate normal distribution, and (c) an accounting for "consistent responses" (see Duncan 1979) must be made in order to describe the joint distribution of these three items. Outwardly the four examples appear quite different from one another, and distinct bodies of methodology have been developed which purport to deal with each problem area from which the examples are taken. Because the log-multiplicative association model presents a unified framework within which these varied problems can be studied, it should be seriously considered as a competitor with other methods of analyzing ordinal data (see Kim [1975] and references cited there). Before the examples are presented, the model will be formulated and briefly discussed.

THE LOG-MULTIPLICATIVE ASSOCIATION MODEL

Suppose that a discrete ordinal variable with I categories is cross-classified with an ordinal variable with J categories, producing an $I \times J$ contingency table. Let the observed frequencies in this table be denoted by f_{ij} , and let the expected frequencies under some model be denoted by F_{ij} , for $i = 1, \dots, I$; $j = 1, \dots, J$. The log-multiplicative model of interest here can be described in a variety of ways, and it is instructive to consider at least some of these alternatives in order to gain understanding of the uses to which the model can be put. The first formulation is

$$F_{ij} = \tau \tau_i^R \tau_j^C e^{\phi \mu_i \nu_j}, \quad (1)$$

where ϕ , μ_i , and ν_j are powers of e , the base of natural logarithms.² The τ_i^R pertain to "row effects" on the F_{ij} , and they correspond to "fitting" the

² The notation used here differs slightly from that used in Goodman (1979).

row marginal in the table of the F_{ij} (i.e., these parameters ensure that $\hat{F}_{i.} = f_{i.}$). Similarly, the τ_j^O pertain to the "column effects" on the F_{ij} , and τ pertains to fitting the sample size n . The τ , τ_i^R , and τ_j^O themselves are of no interest here; the focus is on the "interaction" or "association" between the two variables which is captured by the terms that appear as powers of e in equation (1).

Taking logarithms of equation (1) gives

$$\log F_{ij} = \lambda + \lambda_i^R + \lambda_j^O + \phi\mu_i\nu_j, \quad (2)$$

where $\lambda = \log \tau$, $\lambda_i^R = \log \tau_i^R$, and $\lambda_j^O = \log \tau_j^O$. If the μ_i and the ν_j are constants, not to be estimated from the data, and if they depict the category scores for the row variable and the column variable, respectively, then the model is identical to Haberman's (1974) model of linear-by-linear interaction. The product of the scores ($\mu_i\nu_j$) defines a linear-by-linear interaction term, and ϕ would be the coefficient describing its effect on the $\log F_{ij}$. If the μ_i and the ν_j are given, the model is log-linear in the unknown parameters, λ , λ_i^R , λ_j^O , and ϕ , and standard methods of dealing with log-linear models can be directly applied to it (Bock and Yates 1973; Haberman 1979; Goodman 1979).

There is an element of indeterminacy regarding both the row scores μ_i and the column scores ν_j : only the ratios of distances between scores have any importance for the model. Consider for a moment the row-category scores μ_1, \dots, μ_I , scores that can be assigned to the categories of the ordinal row variable which would perforce make it an interval variable. Let the distance between the first two categories be $d_1 = \mu_2 - \mu_1$, let the distance between the second two categories be $d_2 = \mu_3 - \mu_2$, and let their ratio be $d = d_2/d_1$. Clearly, if the location of the μ_i is changed by subtracting a constant, that is, $\mu_i^* = \mu_i - a$, the first and second intervals still have distances d_1 and d_2 , respectively. The model in equation (2) is not altered by a location change in the μ_i ; it can be shown that ϕ would still be the proper interaction parameter. If the location and the scale of the μ_i are changed by taking $\mu_i^* = (\mu_i - a)/b$, then the first distance $\mu_2^* - \mu_1^* = d_1/b \neq d_1$, and the second distance $\mu_3^* - \mu_2^* = d_2/b \neq d_2$. However, the ratio of distances is $(d_2/b)/(d_1/b) = d_2/d_1 = d$, being unaffected by location and scale changes. The model in equation (2) is unaffected by a scale change in the μ_i , except that $\phi^* = b\phi$ would be the relevant interaction parameter.

Now suppose that the μ_i and the ν_j are taken as parameters to be estimated, instead of constants chosen a priori to depict the category scores in question. The model of equations (1)–(2) is not in this case a log-linear model, and methods discussed by Goodman (1979) or Clogg (1982) are required to estimate its parameters. Inspection of equation (2) makes it clear that the parameters μ_i and ν_j can be regarded as category scores consistent with a model of linear-by-linear interaction. Thus, if the model were

true, and if it had been assumed that the association between the variables could be described by linear-by-linear interaction, the parameters μ_i and ν_j could be used to scale the two variables in question, recognizing that only ratios of distances are identifiable.

An alternative way to describe the model in equation (1) is obtained by considering the odds ratios

$$\theta_{ij} = (F_{ij}F_{i+1,j+1}) / (F_{i,j+1}F_{i+1,j}), \quad (3)$$

for $i = 1, \dots, I - 1$; $j = 1, \dots, J - 1$. Each odds ratio measures the association between the row and column variables in a particular region of the table, namely, the association in the 2×2 subtable formed by taking adjacent rows i and $i + 1$ and adjacent columns j and $j + 1$. The full set of $(I - 1) \times (J - 1)$ such odds ratios can be regarded as containing all the information about the association between the variables, and so a model that explains the variability in this set of odds ratios is desired. Using equations (2) and (3), we obtain

$$\log \theta_{ij} = \phi(\mu_{i+1} - \mu_i)(\nu_{j+1} - \nu_j). \quad (4)$$

Thus, the model is log multiplicative in terms of the association indexes θ_{ij} , and the "distances" appear explicitly as multipliers of ϕ . We note that, if the ordering of the row variable is correct, then $\mu_1 \leq \dots \leq \mu_I$, and the distances $\mu_{i+1} - \mu_i$ will all be nonnegative, with a similar statement applying to the ν_j . The product $(\mu_{i+1} - \mu_i)(\nu_{j+1} - \nu_j)$ will be nonnegative everywhere, if the correct ordering of categories is used for each variable, and the association will be positive, negative, or nil according to whether ϕ is positive, negative, or nil. The parameter ϕ can be regarded as the "overall" association between the row and column variables, and only the distances between adjacent categories are assumed to account for differences among regions in the association actually observed. For example, consider the case in which j is fixed and let $d_i = \mu_{i+1} - \mu_i$. Then we could write $\log \theta_{ij} = \phi_j d_i$, where $\phi_j = \phi(\nu_{j+1} - \nu_j)$. In the first set of adjacent row categories, the association would be described by $\phi_j d_1$, while in the second set of adjacent row categories the association would be described by $\phi_j d_2$. The differing distances (d_1 vs. d_2) are assumed to account entirely for differences in $\log \theta_{1j}$ and $\log \theta_{2j}$ under the model. Thus, if only the estimates of the row variable scores μ_i are of interest and the column variable scores ν_j are not relevant, the model can be regarded as allowing curvilinearity in the association between the row and column variables, with the ϕ_j describing the association for different levels of the column variable.

An interesting and useful property of the model being discussed is that it is invariant under switches in the categories of the row and/or column categories (Goodman 1979). This means that (a) the χ^2 statistics measuring the fit of the model to data are not altered by switching cat-

egories, (b) the parameter ϕ is unchanged (in absolute value) when categories are switched, and (c) relative distances (and orderings) between row scores and between column scores are unchanged when categories are switched. To illustrate the last point, suppose that distances between row categories 1 and 2 and between row categories 2 and 3 are of interest. Under the model, parameters μ_1 , μ_2 , and μ_3 would convey the information about these distances, and the quantity $d = (\mu_2 - \mu_1)/(\mu_3 - \mu_2)$ is the ratio of the distances. If categories 1 and 2 were switched, the scores would appear in the model in the order 2, 1, 3, and the parameters could be written as μ_2^* , μ_1^* , μ_3^* . The quantity $d^* = (\mu_2^* - \mu_1^*)/(\mu_3^* - \mu_2^*)$ will equal d , the relative distance measure obtained for the original ordering. This is a very important property of the log-multiplicative model, and it is a property that is not satisfied with the log-linear model (the linear-by-linear interaction model) that has been used frequently to analyze the cross-classification of ordinal variables.

THE EFFECTS OF QUESTION WORDING ON RESPONSE DISTRIBUTIONS

Table 1 presents responses to two similar questions on the fairness of the courts' treatment of criminals. The source is the 1974 General Social Survey (Davis 1978), in which a randomization technique was used to determine which individuals would be given which question. There is no other apparent difference between the contexts in which each question was used, implying that the observed differences in the response distributions can be

TABLE 1
ATTITUDE TOWARD TREATMENT OF CRIMINALS BY THE
COURTS, 1974 GENERAL SOCIAL SURVEY

RESPONSE	QUESTION WORDING			
	A*		B†	
	N	Percentage	N	Percentage
Too harshly.....	42	5.6	33	4.5
About right.....	72	9.6	44	6.0
Not harshly enough...	580	76.9‡	436	59.7‡
Don't know§.....	51	6.8	210	28.7
No answer.....	8	1.1	8	1.1
Total.....	753	100.0	731	100.0

* The question asked was, "In general, do you think the courts in this area deal too harshly or not harshly enough with criminals?"

† The question asked was, "In general, do you think the courts in this area deal too harshly, or not harshly enough with criminals, or don't you have enough information about the courts to say?"

‡ Percentage adjusted to ensure sum = 100.

§ For wording B, the "don't know" should be replaced by "not enough information to say."

attributed solely to question wording, apart from considerations of sampling error. The actual questions used appear in the notes to table 1; a careful reading of them indicates that the response, don't know (DK), was probably discouraged by the first wording but encouraged by the second. The second wording (form B) appears to encourage the DK response in much the same way as the "filter" design discussed by Schuman and Presser (1978), while the first wording (form A) is consistent with standard techniques of survey design. The differences in the two response distributions are dramatic, with the first estimating the proportion of the population who think the courts are "not harsh enough" at 77%, while the second estimates the same proportion at 60%. The DK response is 7% in the first and 29% in the second.

A variety of statistical methods can be used to reconcile the two observed distributions (see Clogg 1980). The approach taken here is to assume that there is an underlying continuum on the attitude in question, and that each question performs differently in locating points on that continuum. An association model that takes these assumptions into account seems appropriate, and the strategy followed consists in the following steps. (1) Delete the response, no answer (NA), from consideration, but retain the DK. The NA response does not appear to represent a source of discrepancy between the two distributions (each wording received 1.1% of NA responses). However, the DK is very important and should somehow be included in an attempt to reconcile the two distributions. (2) Assume that the points along the underlying continuum of the attitude can be represented by scores $\mu_1, \mu_2, \mu_3, \mu_4$, the last quantity referring to the score for the DK. The assumption here is that the DK is somehow a measure of the strength of the attitude—it does not represent a "nonattitude."³ (3) Assume that each question wording yields a different set of scores μ_1, \dots, μ_4 , and that the differences in the scores can be attributed solely to the difference in the wording. (4) Choose a "criterion" variable, or "instrument," with which the attitude variable can be cross-classified. The schooling variable was used for this example; it was coded as less than 12, 12, 13–15, 16, and 17 or more years, yielding a five-category ordinal variable.⁴ (5)

³ That the percentage of DK responses differs so widely from one form to the other (7% vs. 29%) lends some credence to the view that the DK response represents something other than a nonattitude.

⁴ Since years of schooling is an interval-level variable, we are not interested in estimating distances between categories of this variable. Since the category scores of only one variable are of interest, the model of eq. (1), (2), or (4) can be interpreted as allowing the association between the attitude variable and the schooling variable to depend on the level of schooling (allowing curvilinearity), but once proper category scores for the attitude variable are assigned, this association does not depend on the level of attitude. Stanley Presser notes (personal communication) that a different categorization of the schooling item could lead to a different inference.

Apply a conditional association model (Clogg 1982) which constrains the schooling effects on the association (the v_j of the preceding section) to be identical in each cross-classification, but allows the response variable scores (the μ_i) to differ. The assumption is that schooling is a proper instrument, which implies that the association of the response with the schooling variable is adequately described by linear-by-linear interaction. (This last assumption is critical, and although we can use data in some cases to reject it, no completely convincing empirical test of its validity can be made. Further comments on the rationale for choosing instruments are presented in the final section.)

Using statistical methods described in Goodman (1979) or Clogg (1982), we find that the log-multiplicative association model just described has a likelihood-ratio χ^2 statistic $L^2 = 18.13$ on 15 degrees of freedom, certainly an acceptable fit. The scores obtained for the response variable are reported in table 2, where certain constraints have been used to interpret the differences in scores that result. For question-form A, I set $\hat{\mu}_1$ at zero and obtained $\hat{\mu}_2 = .183$, $\hat{\mu}_3 = .290$, and $\hat{\mu}_4$ (for the DK) = .288. The distances $\hat{d}_i = \hat{\mu}_{i+1} - \hat{\mu}_i$ are thus .183, .107, -.002, showing that the DK is virtually indistinguishable from the response, not harshly enough, for this wording. (Perhaps the DKs for this wording were leaning toward the response, not harshly enough, but for whatever reason did not wish to commit themselves to this "conservative" response.) The scale of the estimated scores was not further modified; the zero restriction for $\hat{\mu}_1$ was the only restriction imposed. The ratios of distances $r_i = d_{i+1}/d_i$ were estimated at .58 and -.02, and I hasten to add that these two quantities would be invariant under any location or scale change in the scores.

For question-form B, an altogether different pattern emerges. To render the scores for this form comparable to those for the first, a location restric-

TABLE 2
ESTIMATED CATEGORY INTERVALS FOR THE
ATTITUDE VARIABLE IN TABLE 1

RESPONSE	QUESTION WORDING	
	A	B
Too harsh.....	.000*	-.266
About right.....	.183	.025
Not harshly enough.....	.290	.464
Don't know.....	.288	-.021
Mean†.....	.26	.26

* The value .000 is arbitrary. See text for explanation.

† Category scores for wording B were adjusted to ensure that equal means would be obtained. Calculations were carried out to more digits than are reported.

tion was first employed. Since both questions measure the same attitude, albeit in different ways, it seems reasonable to expect that the mean score (the actual weighted mean) should be identical for each form. The scores for question-form B were adjusted, while preserving differences, so that the mean for each form was $\bar{X} = .26$. This yields $\hat{\mu}_1 = -.266$, $\hat{\mu}_2 = .025$, $\hat{\mu}_3 = .464$, $\hat{\mu}_4 = -.021$ as the estimated category scores for form B. Thus, the DK response here falls between "too harsh" and "about right," being closer to about right. For this wording, then, it would be reasonable to place the DKs with the neutral response, about right, whereas such a placement would be entirely unacceptable for the first form. Note that the range of the scores is greater for question-form B ($-.266$ to $.464$, for a range of $.730$) than for question-form A ($.290$), implying that the response, too harsh, is further to the left on the continuum for form B than for form A, with a corresponding statement applying to the response, not harshly enough. For form B, the distances between the responses, too harsh and about right, and between about right and not harshly enough (d_1 , d_2) are estimated at $.291$ and $.439$, with the ratio being $.439/.291 = 1.51$ (as compared with $.58$ for form A); this ratio shows in different terms how the responses to form B are spread out along the underlying continuum of the attitude.⁵

In sum, the DK response appears to be indistinguishable from the conservative response, not harshly enough, in form A, but it appears to be nearly the same as the neutral response, about right, in form B. Form B yields a set of scores with greater range (more variance). The very different response distributions in table 1 can be reconciled using the log-multiplicative model, and the scores in table 2 could be used to construct an interval-level variable for further substantive analysis. All of these deductions are valid only to the extent that (a) the schooling variable is a proper instrument, implying that (b) the relationship between the attitude and schooling is adequately described by the linear-by-linear interaction model of equation (1) or (2). I defer a critical appraisal of these assumptions until other examples are discussed.

⁵ The reader will notice that only one restriction was used for each set of $\hat{\mu}_i$ in table 2; the $\hat{\mu}_i$ for question-form A were restricted by setting $\hat{\mu}_1 = 0$, and the $\hat{\mu}_i$ for question-form B were restricted by adjusting the scores so that their mean was equal to that obtained for the $\hat{\mu}_i$ of question-form A. However, the algorithm used to obtain the results began with identical initial values of $\hat{\mu}_i$ for each wording. Thus, the final values for $\hat{\mu}_i$ for both groups (the maximum-likelihood estimates) are comparable across groups with just one restriction imposed on each set. If $\hat{\mu}_1$ is set at zero for the first form, the calculated value of $\hat{\mu}_1$ for the second form ($= -.266$) can be said to indicate a position on the underlying continuum .266 units less than the arbitrary score $\hat{\mu}_1 = 0$ for the first form. Similar comments apply to the other $\hat{\mu}_i$ when compared across question wordings. Such a strategy appears much more fruitful than arbitrarily imposing scale and location restrictions on both sets of estimates. A referee notes that confidence intervals on the distance measures would be useful at this point, but unfortunately the necessary standard errors are difficult to come by.

ASSIGNING A METRIC TO AN ORDINAL VARIABLE:
THE HAPPINESS ITEM

A question that has always appeared on the General Social Survey is the "overall happiness" item, which elicits the ordered responses, not too happy, pretty happy, and very happy. We suspect that many researchers assume that scores 1, 2, and 3 can be used for these response categories, and they then proceed in their statistical analysis as if this arbitrarily scored variable were a proper interval-level variable. Table 3 cross-classifies this trichotomy by sex and schooling for the 1977 General Social Survey, using a slightly different categorization of the schooling variable than was used previously. These data were studied in Clogg (1982) from the point of view of conditional association models, and the reader is referred to this source for a detailed analysis using a variety of different models. The schooling variable is used as an instrument once again, and sex is used as a group variable in order to validate partially the inferences drawn about the scores of the happiness item. The responses to the happiness question do not appear to have different meanings for the sexes; I can think of no compelling reasons why the response, not too happy, for example, would indicate different levels of happiness for males than for females, even though there may indeed be different overall levels of happiness associated with each sex.

The model of conditional independence (happiness independent of schooling for each sex) yields $L^2 = 48.88$ on 12 df, a very significant result. The model allowing schooling scores to differ across sexes (allowing the happiness-schooling association to depend on the level of schooling in different ways for each sex), but retaining homogeneous happiness scores, gives $L^2 = 6.15$ on 5 df. To obtain some sense of how different the happiness scores might be across the sexes, a model allowing these scores to differ as well

TABLE 3
CROSS-CLASSIFICATION OF U.S. SAMPLE ACCORDING TO
REPORTED HAPPINESS AND YEARS OF SCHOOLING,
BY SEX, 1977 GENERAL SOCIAL SURVEY

REPORTED HAPPINESS	YEARS OF SCHOOL COMPLETED			
	<12	12	13-16	17+
Males				
Not too happy.....	40	21	14	3
Pretty happy.....	131	116	112	27
Very happy.....	82	61	55	27
Females				
Not too happy.....	62	26	12	3
Pretty happy.....	155	156	95	15
Very happy.....	87	127	76	15

was fitted, yielding the negligibly different L^2 of 5.91 on 4 df. If the model with heterogeneous happiness scores had provided a significant improvement in fit, the improvement would have indicated that the original model could not be used to estimate category scores for the happiness item. The absence of such an improvement adds credibility to the model and to the happiness scores obtained from it.

The model¹ arrived at can be described in terms of equation (4) as

$$\log \theta_{ij}^m = \phi^m(\mu_{i+1} - \mu_i)(\nu_{j+1}^m - \nu_j^m),$$

$$\log \theta_{ij}^f = \phi^f(\mu_{i+1} - \mu_i)(\nu_{j+1}^f - \nu_j^f),$$

in obvious notation. Note that the level of overall association between schooling and happiness is allowed to differ between the sexes (ϕ^m vs. ϕ^f), and the association is allowed to depend as well on the level of schooling in different ways for the sexes (since the ν_j^m are allowed to differ from the ν_j^f). But the happiness scores are constrained to be homogeneous across sex groups, and it is arguable that this should be the case. The reader should note that the application of the log-multiplicative association model in the present example differs from that in the previous one. The chief difference is the use of the sex variable as a means of strengthening the inferences drawn, giving more opportunity to reject the model as a device for estimating happiness scores.

The estimates of the μ_i for the happiness scores appear in table 4, and the estimated distances are $\hat{\mu}_2 - \hat{\mu}_1 = .79$ and $\hat{\mu}_3 - \hat{\mu}_2 = .26$.⁶ The distance between the responses, not too happy and pretty happy, is about three times the distance between the responses, pretty happy and very happy ($.79/.26 = 3$). These scores, or any others that preserve the estimated ratio of distances, could, under the truth of this model, be used to scale the

TABLE 4
CATEGORY SCORES FOR THE HAPPINESS VARIABLE IN TABLE 3, OBTAINED FROM THE CONDITIONAL ASSOCIATION MODEL WITH HOMOGENEOUS ROW EFFECTS AND HETEROGENEOUS COLUMN EFFECTS

Response	Category Score $\hat{\mu}_i$	$\hat{\mu}_{i+1} - \hat{\mu}_i$
Not too happy.....	1.35	
Pretty happy.....	2.14	.79
Very happy.....	2.41	.26

⁶ No restrictions were imposed on these parameter estimates. The reader should bear in mind that only the ratios of distances are invariant under various parameterizations of the model.

happiness variable. It can be noted that the hypothesis of equal intervals between the happiness scores gives $L^2 = 11.64$ on 6 df. The difference $11.64 - 6.15 = 5.49$, a single degree-of-freedom χ^2 variate under the hypothesis of equal spacing, is statistically significant. This implies that the ratio 3 ($= .79/.26$) is significantly different from one. Suffice it to say that the estimated distances between the categories of the happiness variable appear credible, and the somewhat more involved statistical analysis of rival models (see Clogg 1982) does not give reason to question the results. A technique like that used here can be used to scale any ordered variable that appears in sample surveys, given proper instruments with which these ordinal variables can be calibrated.

ESTIMATING SCALE SCORES FOR GUTTMAN-TYPE RESPONSE PATTERNS

Guttman scaling procedures have been an accepted part of sociological research for three decades. A variety of statistical methods can be used to assess the conformity of data to the Guttman model (Goodman 1975; Clogg and Sawyer 1981; Lazarsfeld and Henry 1968), although convincing methods of assigning scores to response patterns obeying a Guttman model, or one of its stochastic variants, have not been so easy to develop. Lazarsfeld's latent distance model (Lazarsfeld and Henry 1968) is one approach by which scale scores have occasionally been constructed, but most available software packages merely form summated scales (summing up the items) to arrive at an "interval" variable for substantive analysis. The log-multiplicative association model represents an alternative method by which scale scores can be assigned, although, as in the previous examples, the validity of the scale scores so obtained depends on the availability of a suitable instrument.

To illustrate this technique, three items on the attitude toward legal abortion were considered. The items pertained to whether abortion should be legal for a woman who is poor (POOR), for a woman who is single (SINGLE), or for a woman who wants no more children (NOMORE). Assuming that these items indicate different points along an underlying continuum of the attitude in question, Guttman's model predicts that the four response patterns ([1,1,1], [1,1,2], [1,2,2], [2,2,2]) would contain all respondents.⁷ (The ordering of items was POOR, SINGLE, and NOMORE,

⁷ No test of the conformity of the data to Guttman's model was performed. The test described by Goodman (1975) for his probabilistic variant of the Guttman model cannot be directly applied to the 2³ table. However, Goodman's parameter π_0 , measuring the proportion of respondents "intrinsically unscalable," has an estimate of .32 (Goodman 1975, eq. [A.7]). This index of scalability is reasonably small, especially in comparison with the values obtained for this quantity in other applications of the Goodman scale model (Clogg and Sawyer 1981). We shall assume that these three items are Guttman scalable, implying that they measure a unidimensional attitude.

and 1 refers to the yes response). In the $2 \times 2 \times 2$ class-classification of these items, error responses were found, and in circumstances like these it can be difficult to defend any procedure for assigning scale scores. However, an application of the log-multiplicative association model produces scores that can be defended in terms of the model, and the required assumptions for its use appear to be no more stringent than those used implicitly to construct scale scores by conventional ad hoc methods.

Consider the fivefold variable with one category for each of the four scale-response patterns and an additional category denoting error-response patterns.⁸ The exact ordering of the categories of this variable is open to dispute, particularly since it is not clear where the error-response patterns should be placed. What is needed is an instrument with which this fivefold variable can be cross-classified, producing a contingency table that exhibits linear-by-linear interaction. The instrument chosen here was the item measuring the attitude on premarital sex, the assumption being that the abortion attitude should be associated with this variable in a way that can be described adequately by this model.

Table 5 presents the cross-classification of interest; the notes describe the items used. Note that no other variable (e.g., sex) is used here to "validate" results. This 5×4 table can be studied directly with the techniques in Goodman (1979). The independence model yields $L^2 = 236.34$ on 12 df, while the log-multiplicative association model yields a near perfect fit, $L^2 = 5.55$ on 6 df. Table 6 presents the log-odds-ratios observed and predicted

TABLE 5
CROSS-CLASSIFICATION OF ABORTION ATTITUDE BY ATTITUDE ON
PREMARITAL SEX, 1977 GENERAL SOCIAL SURVEY

ABORTION ATTITUDE RESPONSE PATTERNS*	PREMARITAL SEX†				TOTAL
	1	2	3	4	
0. Error responses...	44	11	38	62	155
1. (1,1,1).....	59	41	147	293	540
2. (1,1,2).....	23	11	13	27	74
3. (1,2,2).....	27	8	16	27	78
4. (2,2,2).....	258	57	105	110	530
Total.....	411	128	319	519	1,377

* The three abortion items were as follows. Should legal abortion be available to a woman: "If she is married and does not want any more children?" (NOMORE); "If the family has a very low income and cannot afford any more children?" (POOR); "If she is not married and does not want to marry the man?" (SINGLE). The Guttman-scale ordering for these items, based on inspection of the single-item marginals, was POOR-SINGLE-NOMORE; a response pattern of (1,1,2) corresponds to a "yes" for both POOR and SINGLE and a "no" for NOMORE.

† The question was, "Do you think [premarital sex] is always wrong, almost always wrong, wrong only sometimes, or not wrong at all?" The responses are coded 1-4, respectively.

⁸ All the error-response patterns were condensed into one category, but the methods discussed here do not require this condensation.

under the model, showing in different terms how well the model fits. (There do appear to be some log-odds-ratios that are not predicted well under the model, but the reader should bear in mind that the counts in some of the 2×2 subtables are rather small.)

Table 7 presents the scale scores estimated under the model, including a score for the error-response patterns.⁹ If the five categories are denoted by 0, 1, 2, 3, 4, then the suggested ordering is 1, 0, 2, 3, 4. The error-response patterns are estimated to indicate a position somewhere between the (1,1,1) and the (1,1,2) patterns. Given these category scores, the numbers .507, .049, -.064, -.101, and -.391 could be used to create an interval-level attitudinal variable, with imputed distances of .458, .113, .037, and .290. The ratios of these distances are $.458/.113 = 4.05$, $.113/.037 = 3.05$, and $.037/.290 = .13$. Under the model, it would be entirely inappro-

TABLE 6
OBSERVED AND EXPECTED LOG-ODDS-RATIOS
FOR TABLE 5

ABORTION ATTITUDE RESPONSE PATTERNS	PREMARITAL SEX		
	1 vs. 2	2 vs. 3	3 vs. 4
0 vs. 1.....	1.02 (.57)	.04 (.36)	.20 (.30)
1 vs. 2.....	-.37 (-.71)	-1.11 (-.46)	.04 (-.37)
2 vs. 3.....	-.48 (-.05)	.53 (-.03)	-.21 (-.02)
3 vs. 4.....	-.29 (-.36)	-.08 (-.23)	-.48 (-.19)

NOTE.—Estimated expected log-odds-ratios are shown in parentheses.

TABLE 7
ESTIMATED SCALE SCORES FOR
THE ABORTION ATTITUDE
RESPONSE PATTERNS

Abortion Attitude Response Pattern	Scale Score*
0. Error.....	.049
1. (1,1,1).....	.507
2. (1,1,2).....	-.064
3. (1,2,2).....	-.101
4. (2,2,2).....	-.391

* Scores constrained to sum to zero.

⁹ A location restriction was imposed on the $\hat{\mu}_i$ in table 7 by restricting $\sum_i \hat{\mu}_i = 0$.

priate to use scores that reflected an assumption of equal intervals (even disregarding the error-response patterns). Once again it must be noted that these inferences are only as good as the model, and the application of the model to obtain category scores depends on the suitability of the instrument (premarital sex) used.

For completeness, it can be noted that the estimates of the v_j , $j = 1, \dots, 4$, were $-.280$, $-.048$, $.102$, and $.225$, respectively. If it had been decided to use the association in table 5 to calibrate the premarital sex item, these scores could have been used for this purpose. However, a somewhat stronger assumption would have to be made: that the entire bivariate relationship could be simply described by linear-by-linear interaction. It is interesting to examine the model applied to the table with some categories switched. For example, if categories 3 and 4 of the premarital sex item are switched, the log-multiplicative model applied to this modified table still gives $L^2 = 5.55$. The estimates of the premarital sex scores are now $\hat{v}_1 = -.448$, $\hat{v}_2 = -.076$, $\hat{v}_4 = .361$, and $\hat{v}_3 = .164$. These estimates are different from those obtained for the original table, but the relative distances between categories are still the same. For example, $(\hat{v}_3 - \hat{v}_2)/(\hat{v}_4 - \hat{v}_3) = 1.22$ for either set of estimates. This is a demonstration of the invariance property discussed early in this paper (see Goodman 1979), and similar comments apply to other types of category switches.

ANALYSIS OF ORDINAL INDICATORS

Table 8, obtained once again from the 1977 General Social Survey, is a three-way cross-classification of ordinal indicators of satisfaction with life. The specific indicators used pertain to satisfaction with residence, family, and hobbies, and a fourfold categorization of responses was used. These data would be conventionally approached from the framework of the common-factor model (see, e.g., Jöreskog and Sörbom 1979), but they might be approached as well from the latent class framework (Clogg 1979). Here they will be studied from the point of view of the log-multiplicative association model, and the partial association models of Clogg (1982) become relevant. Let R , C , and L denote the three variables under consideration, and let i , j , and k denote the category indexes, ranging from 1 through 4. To describe the partial association between R and C , the partial odds ratios $\theta_{ij(k)}^{RC} = (F_{ijk}F_{i+1,j+1,k})/(F_{i,j+1,k}F_{i+1,j,k})$ are relevant, and similar partial odds ratios can be defined for the R - L and C - L partial association. The approach begins by assuming that each variable is a measure of an underlying continuum (e.g., satisfaction with residence), and we let μ_i , v_j , and ξ_k denote the category scores for each variable. (Note that we have not made the assumption that each indicator is somehow a fallible measure of

TABLE 8

CROSS-CLASSIFICATION OF U.S. SAMPLE ACCORDING TO
THREE INDICATORS OF SATISFACTION WITH LIFE,
1977 GENERAL SOCIAL SURVEY

<i>L</i> = ,	<i>R</i> =	<i>C</i> = 1	<i>C</i> = 2	<i>C</i> = 3	<i>C</i> = 4
1, 1,	1.....	76	14	15	4
1, 1,	2.....	32	17	7	3
1, 1,	3.....	64	23	28	15
1, 1,	4.....	41	11	27	16
2, 2,	1.....	15	2	7	4
2, 2,	2.....	27	20	9	5
2, 2,	3.....	57	31	24	15
2, 2,	4.....	27	9	22	16
3, 3,	1.....	13	6	13	5
3, 3,	2.....	12	13	10	6
3, 3,	3.....	46	32	75	20
3, 3,	4.....	54	26	58	55
4, 4,	1.....	7	6	7	6
4, 4,	2.....	7	2	3	6
4, 4,	3.....	12	11	31	15
4, 4,	4.....	52	36	80	101

NOTE.—Variables *L*, *R*, and *C* refer to satisfaction with hobbies, family, and residence, respectively. Variable codes are 1 (a fair amount, some, a little, or none), 2 (quite a bit), 3 (a great deal), and 4 (a very great deal).

a "common" factor.) The model of interest can be expressed in terms of the partials as

$$\log \theta_{ij(k)}^{RC} = \phi^{RC}(\mu_{i+1} - \mu_i)(\nu_{j+1} - \nu_j) ,$$

$$\log \theta_{i(j)k}^{RL} = \phi^{RL}(\mu_{i+1} - \mu_i)(\xi_{k+1} - \xi_k) ,$$

$$\log \theta_{(i)jk}^{CL} = \phi^{CL}(\nu_{j+1} - \nu_j)(\xi_{k+1} - \xi_k) .$$

Note that the overall levels of partial association are allowed to differ between pairs of variables. Note in addition that the model is positing linear-by-linear, first-order interaction between pairs of variables, subject to the appropriate scores being assigned to the response categories. Finally, note that the μ_i in the first and second equations are identical, the ν_j in the first and third equations are identical, and the ξ_k in the second and third equations are identical. The model is described in Clogg (1982) as the homogeneous row-, column-, and layer-effects partial association model, and a more general model would not constrain the category scores to be homogeneous across types of partials. (E.g., the μ_i of the first equation could be allowed to differ from the μ_i of the second equation.)

Attempts to apply the model described by the three equations immediately above to the data in table 8 were unsuccessful. In fact, a model that allowed the category scores to differ across types of partials gave an L^2 of 123.59 on 54 df, suggesting that the more restrictive model would also fail

to fit. However, with the consistent responses ([1,1,1], [2,2,2], [3,3,3], [4,4,4]) blanked out, satisfactory results were obtained.¹⁰ The model for the incomplete table so constructed gave $L^2 = 49.31$ on 41 df, certainly an acceptable fit. The conclusion must certainly be that an appropriate model for the association of these ordinal indicators has to take account of the fact that far too many respondents answer all items consistently, invalidating any model which does not take this information into account. Results of Goodman (1981a) indicate that, if the data were adequately described by the (trivariate) normal distribution, the model for the complete table just discussed would provide an acceptable fit. But the model provides an adequate description of the data only when the incomplete table is used. Moreover, the hypothesis of equal intervals can be rejected (even when the consistent responses are deleted), since $L^2 = 103.95$ on 47 df for this model. The common-factor model which assumes equal intervals and multivariate normality, and which makes no allowance for response consistency, simply cannot be defended for these data. I suspect that similar conclusions would be reached if these models were applied to similar survey items on satisfaction indicators (see, e.g., Andrews and McKennell 1980).

The category scores estimated for these three items appear in table 9. No attempt was made to impose scale or location restrictions on these estimates, but nevertheless an anomalous finding was uncovered for the estimated distance between the first and second responses to the family variable (pertaining to "a fair amount, some, a little, or none" and "quite a bit"). The estimates indicate that the position of these two categories must be switched, and this is admittedly a perplexing result. In circumstances like

TABLE 9
PARAMETER ESTIMATES UNDER THE HOMOGENEOUS ROW-, COLUMN-,
AND LAYER-EFFECTS MODEL APPLIED TO TABLE 8
(with Consistent Response Patterns Deleted)

Parameters		Maximum Likelihood Estimate				
Row effects.....	-.19	(-.92)	-1.11	(.78)	-.32	(1.95) 1.62
Column effects.....	-1.09	(.32)	-.77	(1.20)	.43	(1.00) 1.43
Layer effects.....	-.92	(.03)	-.89	(1.17)	.29	(1.23) 1.51

NOTE.—Figures in parentheses are differences.

¹⁰ Actually, for some models applied to this table it suffices to "blank out" only the extreme response consistency patterns ([1,1,1], [4,4,4]). Inferences drawn here would not be markedly different with this alternative formulation of the model.

these, the model must be called into question, but as an expedient we might simply average the anomalous scores.¹¹

In order to use the scores in table 9 for substantive work, it is necessary to impose some restrictions on them. Table 10 presents one such attempt, in which the score for the fourth category ("a very great deal") is set arbitrarily at one for each item. Because of the anomalous finding (cf. table 9) for the first-category score on the family item, this score was arbitrarily adjusted as well, setting it and the second score to the mean of these two scores. It can be noted that the distances between the first two categories are, on the whole, much smaller than the distances between either the second and third or the third and fourth categories. A "summated scale" of the three items might be constructed by using the appropriate sums of scores in table 10, or these scores might be used in a factor-analytic model to obtain a composite index of a different sort. This is by no means a final solution to the problem of ordinal indicators, but the association models used here are at least suggestive of the potential worth of pursuing the subject further.

DISCUSSION

The four examples considered briefly in this paper were intended to illustrate several problems to which the log-multiplicative association model could be applied. Each situation demanded a slightly different type of model, and it is instructive to consider the way that these different models were formulated and utilized. The first example pertained to reconciling two different question wordings measuring the same attitude (*viz.*, the attitude toward the courts' treatment of criminals). It was assumed that (*a*) schooling should be associated with the attitude in the same way for each question wording, given an appropriate accounting for category scores; (*b*)

TABLE 10
CATEGORY SCORES FOR THE ORDINAL INDICATORS IN TABLE 8

ITEM	SCORE FOR CATEGORY			
	1	2	3	4
Family.....	-1.27*	-1.27*	-.95	1.00†
Residence.....	-1.52	-1.20	.00	1.00†
Hobbies.....	-1.43	-1.40	-.23	1.00†

* Arbitrarily set equal to the mean score for categories 1 and 2.

† Arbitrarily set equal to one.

¹¹ Techniques described in Goodman (1981b) could be extended to the three-variable situation in order to examine whether the switch in categories is statistically significant. To save space, these matters will not be discussed here.

schooling could serve as an instrument with which the category scores could be estimated; and (c) the mean score for the attitude measured should be the same for each question wording. These assumptions led to an association model which produced plausible category scores for each response distribution. The model posited heterogeneous attitude-variable scores (across question wordings), and homogeneous schooling effects on the association (across question wordings). The model allowed "curvilinearity" in the association across levels of schooling.

The second example pertained to the trichotomous happiness item, and the schooling variable was once again chosen as an instrument with which to calibrate the ordinal variable in question. A third variable (sex group) was also introduced, giving more opportunity to reject the model as a device for estimating category intervals. The association between happiness and schooling was allowed to depend on sex, and it was also allowed to depend on the level of schooling. But the happiness item scores were assumed to be the same for each sex, an assumption that is tantamount to claiming that the response, very happy, indicates the same level of happiness for each sex. The model posited homogeneous happiness scores (across sex groups), and heterogeneous schooling effects on the association (across sex groups). This model was compared with one alternative, thereby strengthening the inferences drawn.

The third example pertained to an ordered variable that naturally arises from a Guttman scaling situation. For this problem, one instrument (the attitude on premarital sex) was employed to estimate category scores, using the log-multiplicative model for the two-way table. The necessary specification was that the association between the ordinal variable and the instrument should not depend on the level of the ordinal variable, once a proper adjustment for category intervals is made. The association could depend on the level of the attitude toward premarital sex (the instrument). Satisfactory results were obtained, and plausible scores were assigned to the response patterns that arose from the Guttman model.

The fourth example pertained to an analysis of three ordinal variables, considered in terms of their partial relationships with each other. For this situation, no external instrument was brought into the analysis, and the specification of linear-by-linear first-order association was used to calibrate each ordinal variable. An anomalous result occurred for one of the category scores for one of the variables, casting doubt on the model. But the analysis nevertheless showed conclusively that (a) an assumption of equal intervals could be rejected, (b) the assumption of trivariate normality could be rejected, and (c) an accounting for "response consistency" was required before the association among the variables could be described.

Since three of the examples utilized an "instrumental" variable in order to estimate the desired category intervals, it is appropriate to provide some

criteria for the selection of instruments. Of course, if several different instruments were available, it would be prudent to utilize all of them, thereby acquiring some sense of how the estimated category scores depend on the particular instrument chosen. But some criteria can be helpful in deciding which of the available instruments should be used. Even though there is no completely convincing empirical test of the suitability of an instrument, there are several conditions that must be met for the instrument to be valid.

Suppose that the row variable R is the ordinal variable to be calibrated, and the column variable C is the instrument chosen for the application of the model. (If additional variables are also available and relevant—see the first two examples—additional considerations can also be used.) First, there must be association between R and C . If the instrument C is not associated with R , no information is available with which to estimate the category intervals. To see why this is so, consider the expression in equation (4). The case where there is no association between R and C can be equivalently described by saying that $\phi = 0$, or by saying that all the differences $\mu_{i+1} - \mu_i = 0$; thus, there is no way that distances between the categories of R can be estimated.¹² Second, the model must produce estimated category scores which do not violate known ordinality requirements. That is, if it is known a priori that the first I^* of the I categories are naturally ordered from 1, 2, . . . , I^* , then the estimated category scores corresponding to these categories must satisfy the relationship $\mu_1 \leq \mu_2 \leq \dots \leq \mu_{I^*}$. Third, the model used must fit the data to an acceptable degree. If the model does not fit the data, the association between R and C is more complicated than that which can be described by Goodman's row- and column-effects association models, and the calculation of category scores for the R variable would be wholly unjustified. Fourth, the instrument must be chosen with the best substantive and/or theoretical information available about how the R variable is associated with it. If there are serious theoretical reasons why the relationship between R and C cannot be described by linear-by-linear interaction, this information by itself should force the researcher to cast the instrument aside, even if the first three criteria are satisfied. Finally, it may be advisable to exploit information from more than one in-

¹² A related topic of some importance is testing whether $\phi = 0$ under the model of eq. (2). The test statistic for this situation has a rather anomalous sampling distribution (Haberman 1982; Goodman 1981b). As an expedient, I recommend that researchers test initially for association of the row and column variables using the conventional χ^2 test of independence. While this test does not exploit the log-multiplicative model, it does appear to be suitable for practical use. If the researcher does not find strong evidence for association using the conventional χ^2 test, then he or she should be skeptical about the use of the particular instrument to calibrate the ordinal variable. On the other hand, if there is strong evidence for association as judged by application of the conventional test, the researcher can be reasonably assured that (a) ϕ is nonzero and (b) the differences $\mu_{i+1} - \mu_i$ are not all zero. Condition (b) implies that the relevant distances can be estimated.

strumental variable or to exploit a group variable in order to strengthen the inferences. If more than one instrument were available, partial association models could be used to include all the information contained in the complete table describing the (partial) association among items. If a group variable were available, defined so that each group possessed homogeneous category scores on the ordinal variable to be calibrated, this information should certainly be exploited. Such a procedure would allow more opportunity to reject the model and would, thus, strengthen the inferences drawn about distances. Note that the latter strategy was used in the example dealing with the happiness item.

The uses of the log-multiplicative association model surveyed in this paper are only as valid as the assumptions necessary to apply them, and I have tried to address these assumptions explicitly and carefully throughout. Only further work with these methods, involving extensive application to other data, can ultimately attest to the practical utility of the model. I do not believe that the assumptions required for the application of the model are any more stringent than assumptions made in the selection of instrumental variables in causal modeling (e.g., in choosing instrumental variables to identify reciprocal effects, or to identify causal effects of unobservable variables measured with error). I do believe that these methods are a clear improvement over the popular "method" of assuming that ordinal variables are interval level with equal intervals, and it appears that these methods might allow a rapprochement between contingency-table methods for discrete data and linear-model methods for quantitative data.

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Ethnic Enclaves: A Comparison of the Cuban and Black Economies in Miami¹

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Structural hypotheses that link the relative advantage in certain ethnic enclaves to the structure of their economies are tested in a comparative analysis of the Cuban and black businesses in Miami. Findings suggest that the more advantaged community, the Cuban enclave, is characterized by highly interdependent industries, ones which are less dependent on majority industry; the less advantaged community, the black enclave, is characterized by weakly interdependent industries, ones which are more dependent on majority industry. In addition, hypotheses are suggested which link the structuring of enclave economies to traditional concerns with background cultural, historical, and situational influences. The usefulness of input-output analysis and the limitations of secondary data are discussed.

In an earlier paper (Wilson and Portes 1980) the notion of immigrant enclaves, self-enclosed inner-city minority communities, was explored with recent data on Cuban immigrants in Miami. Several hypotheses were developed which attempted to explain the economies and diseconomies of minority enclaves. The purposes of this paper are to elaborate further on notions of the economic structuring of minority enclaves, to search for an appropriate methodology that enables detailed analysis of such structural notions, and to explore the usefulness of secondary data concerning minority-owned firms.

Studies of minority business provide a complementary alternative to the growing body of research devoted to the labor experiences of minorities.

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The latter research has consistently documented the relatively poor life chances of minority workers compared with those of majority workers. Equal employment in majority labor markets is blocked by institutional discrimination; minority employment in inner-city secondary labor markets is characterized by structurally supported barriers which maintain majority-minority inequality. The various approaches that attempt to explain the poor work experiences of minorities note parallels with colonialist exploitation (Carmichael and Hamilton 1967) or the existence of "dual" or "split" labor markets (Doerenger and Piore 1971; Bonacich 1976).

The growth of minority business can be attributed partly to job discrimination in majority labor markets and other disadvantages of minority workers, such as poor English (Light 1980). Perhaps the most interesting aspect of minority entrepreneurship is that a few minority business communities have experienced growth to the point of obtaining a surprising share of consumer and even interindustry markets. Thus, in some cases the alternative of self-employment has been a successful one. A substantial sociological effort in the past decade has attempted to explain the success of the few prosperous ethnic business communities in contrast to the marginality of other minority business communities (Light 1972, 1980; Warren 1977; Bonacich 1973; Suttles 1972).

The strength of this literature lies in delineating cultural, historical, and situational influences which are associated with successful business enterprise. Less attention has been devoted to spelling out the precise business patterns which result from antecedent sociological influences and which in turn result in economic success. While it is natural for sociologists to pay less attention to this more economic side of the picture, a theory of successful ethnic business patterns provides a specific focus for the outcomes of more traditional sociological influences.

This paper attempts a further specification of the economic parameters which mean success or failure in ethnic business enclaves. We ask, To what extent do the structured interconnections among ethnic businesses influence the overall economic well-being of enclave communities? We approach this question with data from the black and Cuban communities in Miami. The Cuban community is the newer of the two. The Cuban business community did not begin to flourish until the early 1960s following the 1959 revolution in Cuba. The black business community began a significant thrust toward development in the 1920s, according to a study by the University of Miami (1976). Despite the earlier beginnings of black business in Miami, in 1972 Cuban-owned firms numbered 2,463 compared with 1,530 black-owned firms, and total Cuban receipts were \$270 million while total receipts for black-owned businesses were only \$75 million (U.S. Bureau of the Census 1974). Clearly Cuban business, with average receipts of \$110,000 per firm, became more prosperous than black business, with average receipts of

\$49,000 per firm, during a single decade. To what extent is the overall structuring of the economies in the two communities responsible for their different prosperity?

In the following sections we present (1) a theoretical framework of alternative structures of enclave economies, followed by related literature and evidence; (2) a discussion of input-output analysis, which is a useful methodology for examining structural economic hypotheses; (3) an empirical analysis of the interdependencies of Cuban-owned business compared with black-owned business in Miami; and (4) a general discussion suggesting interlinking paths of influence between sociological and economic parameters in the determination of the economic well-being of ethnic enclaves.

ALTERNATIVE STRUCTURES OF ENCLAVE ECONOMIES

Theoretical Framework

Our model of economic development in enclaves is taken from the majority economy. Many investigators today argue that the majority economy has bifurcated into two distinct economies to form a "dual economy" (see Averett [1968]; Galbraith [1971]; for an empirical test of the theory of the dual economy, see Oster [1979]). One part, the center economy, is characterized by large, vigorously healthy firms. The other, the periphery, is characterized by economically backward firms with little growth and low-level cash flow. Or, in more radical language, the periphery is characterized by free competition while the center is characterized by monopolies and oligopolies (O'Connor 1973). This division of the economy is, in many respects, the same as Marx's division of capitalists into large and small capitalists (Marx [1867] 1967, vol. 1).

Monopolistic firms have existed throughout the history of American capitalism (see esp. Myers [1907] for an account of them), and over the course of this century they have become a major economic force in America. A number of specific characteristics of monopolistic firms in the center economy distinguish them from firms in the periphery, but we will focus on two key notions: vertical and horizontal integration. Both are methods of neutralizing the effects of competition. Vertical integration, gaining control of sources of supply and sales outlets, occurs *among* categories of industries. "Industry," as used here, refers to firms that provide similar products or services. Horizontal integration, the achievement of cooperative levels of production and pricing strategies, occurs *within* an industry. Mergers are probably the most clearly understood form of vertical and horizontal integration. Other strategies for creating vertical and horizontal integration range from exchanging board members to making contractual ties. Contractual ties exist when one firm agrees to supply a commodity to another

for a specified period of time and at a specified price. For some especially innovative and pioneering work on strategies of vertical and horizontal integration among majority firms, see Burt (1979, 1980).

It is possible to apply the theory of the dual economy to ethnic enclave economies and to form a purely analytical set of hypotheses about the effects of alternative structural forms on enclave economies. The literature which relates to these notions will be reviewed in the next subsection.

Since the majority economy has become organized into two alternative structural forms, the periphery and the center, minority enclaves can have economies that are structured primarily in the image of either structural alternative. Enclaves can be composed of a group of relatively independent firms which compete with each other for supplies and minority consumers, or minority firms can theoretically be arranged in a fairly unified system of vertical and horizontal integration, employing one or more of the strategies for achieving both kinds of integration. In the former case, the enclave resembles, and may indeed be a part of, the periphery in the majority economy. In the latter case, the enclave resembles the center economy and should have many of the advantages which that form of economy enjoys. Stated as a formal hypothesis: *Enclave economies that are vertically and horizontally integrated yield higher initial profits per unit of demand, create higher levels of production in related industries (caused by the initial demand for the first industry's product), pay higher wages, and create more jobs (again because of the initial demand) than enclave economies that are not vertically and horizontally integrated.*

If we are able to confirm this hypothesis, it will suggest that a conceptual distinction is necessary for understanding the nature of small ethnic business enterprise. Clusters of small ethnic businesses may become so arranged as to reproduce the crucial features of the center economy. For such instances, it may not be fruitful to categorize these ethnic businesses in either sector of the dual economy. They may represent a third alternative. Such ethnic enclaves are different from firms in the monopolistic sector in that the latter achieves vertical and horizontal integration within a single firm while the former achieves such structuring by coordinating a cluster of firms. The ethnic enclave is different from the periphery in that the latter is much more "atomized"—that is, less interconnected with other firms in the periphery—than the ethnic enclave. We therefore suggest "enclave economy" as an alternative to the other two forms of economic structure in America.

Related Literature and Supporting Evidence

Wilson and Portes (1980) tested a related hypothesis for immigrants in the Cuban community in Miami. Since the primary labor market services

the core economy and the secondary labor market services the periphery, attainment processes that differ among labor market contexts should indicate underlying structural differences. One of the key individual-level characteristics concordant with labor market contexts is the result of investments in human capital. Investments in human capital do not seem to pay off in a secondary labor market. Findings indicated that immigrant workers employed in the enclave received returns to investments in human capital that were comparable to those received by immigrants working in the primary labor market. On the other hand, investments in human capital did not pay off for immigrants working in the secondary labor market. One implication of these findings is that the enclave labor market is similar to the primary labor market. Furthermore, the findings imply that the underlying structure of the enclave economy might have reproduced features of the core majority economy.

Earlier sociological works have not come to grips explicitly with the structuring of enclave economies that are associated with successful or less than successful communities. Instead, general social dynamics such as community organization or solidarity have been examined. Light (1980) notes that sociological attempts to explain the growth of ethnic business enterprise have generally fallen into two groups, historical/cultural explanations ("cultural theory") and explanations based on more immediate situational factors such as labor market disadvantages ("disadvantage theory").

Support for the importance of vertical and horizontal business organization is implicit in many earlier works. Empirical case studies of the Japanese (Boyd 1971; Petersen 1971), Chinese (Sung 1967; Light 1980), and Koreans (Bonacich 1980; Bonacich, Light, and Wong 1977; Light 1980) in the United States all seem to follow a "center economy" form of enclave development. Bonacich (1973) notes the importance of vertical integration, such as that among Japanese produce growers, wholesalers, and retailers, for the success of "middleman minorities." Light (1972) argues that Japanese-American shoemakers' guilds served the important function of reducing intracommunity competition, determining both prices and the physical locations of shops.

More recently, Cummings's (1980) edited book has provided a rich survey of immigrant business cooperation. The researchers detail the importance of extended family support for businesses, for instance, the Greek pizza businesses (Lovell-Troy 1980). They show that religion was often the basis of mutual benefit societies among Slovak immigrants (e.g., Stolarik 1980). And they demonstrate the relationship between political ideas and cooperatives such as existed in Finnish communities (Karni 1980).

The failure of efforts to generate "black capitalism" can be attributed to a failure to generate center-economy business organization. The National Urban Coalition, organized in 1967, and similar coalitions such as the Co-

alition Development Corporation in New York, which were charged with the task of black community development (all prompted by the race riots of the mid-1960s), were oriented exclusively toward small "atomistic" black businessmen. They saw the main problems in the black communities as the unavailability of "risk" capital and lack of training in business management (or rather, the lack of human capital in general). Vietorisz and Harrison's (1971) program for ghetto development, the formation of community corporations, criticized the "black capitalism" programs for their inattention to the overall structuring of the community economy and argued for community ownership and control of key community resources. Schaffer (1973) demonstrates that the disinvestments in community property by outside white owners in the (black) Bedford-Stuyvesant community in New York create an outflow of income that suffices to offset the inflow of income generated by welfare payments.

The smallness of black business was further documented by O'Connell (1976) in a comparison of total national and national black business receipts for 1967 and 1972. Black receipts were disproportionately concentrated in proprietorships while white receipts were disproportionately concentrated in national corporations. Instead of aiming at removing this imbalance, President Nixon's 1969 Office of Minority Business Enterprise would fund only small businesses (Hetzel 1971).

As a result of the lack of larger businesses in black communities, black workers had to seek employment outside their communities. Heilbrun (1970) and Schaffer (1973) report that unskilled labor was the chief export of the black communities they studied. Indeed, over 80% of Cleveland's Hough ghetto black expenditures go to noncommunity businesses (Oakland, Sparrow, and Stettler 1971). In such communities, welfare and other aid programs serve only to stabilize the poor economy (Fusfeld 1973).

Even though Taylor (1979) indicates that economic organizations and other forms of mutual aid have existed in black enclaves for a century, since World War II such black organizations have faced such problems as central business district expansion. In an effort to support and finance community economic development, a group of developmental economists suggested concentrating development funds and energies on Community Development Corporations (CDCs) (Vietorisz and Harrison 1970, 1971; Blaustein and Faux 1972; Rosenbloom and Marris 1969; and especially the *Review of Black Political Economy's* special issue on CDCs [1973]). All CDCs are operated and controlled by cooperatives from within the community. These corporations orient their sales either toward "social overhead capital" (community schools, housing, transportation, etc.) or toward "directly productive activities" (commodities-oriented business, e.g., manufacturing).

The particular forms that have been suggested for CDCs correspond closely to the hypothesis of this paper. Harrison (1974, p. 19) suggested that the small businesses in minority communities should reorganize into networks of "affiliated independents." Besides capturing the advantages of economies of scale, a characteristic of the center economy, these organizations have a certain amount of advantage over core firms in the sense that, being a collective of independents, they are inherently more flexible in their ability to respond to local market fluctuations. Vietorisz and Harrison's (1970) development strategy for Harlem was designed to build vertical integration within the community. The plan began with gaining control of local retail stores and then gradually "integrating backward." Thus a group of affiliated supermarkets would gradually move into food canning, farming contracts, stationery and office equipment and supplies, printing, and metalworking.

There are a number of econometric methods for measuring aspects of economic structures. We found that the most comprehensive and flexible method for our purposes is input-output (I-O) analysis. Other methods which can complement and elaborate on I-O analysis are income-flows analysis (Schaffer 1973), economic base analysis (Tiebout 1962), and income-expenditure identification (Klein 1969).

INPUT-OUTPUT ANALYSIS

The soundness of I-O models has long been established. It is more useful to review their most salient uses and issues than to recertify the method. Input-output models were used first in studying the interindustry flows of national economies (Leontief 1936). The method has also attracted inter-regional applications (Round 1978; Ghosh 1973), intraregional applications (Isard and Kuenne 1953; Miller 1957), and local applications (Hirsch 1969; Morrison 1973). Ghosh (1964) notes uses confined more strictly to business, such as viewing the dependence of an industry on final demand, making demand projections, and forecasting output.

The uses of I-O modeling to gauge national development are becoming more prevalent, especially for Third World countries. Chenery (1963) argues that I-O models offer the best technique for anticipating interrelated changes in composition of demand, production, trade, and income (e.g., see Sandee's [1959] study of India). Sengupta (1963) studied India's agricultural and industrial sectors to determine how much each should be developed in order to develop the overall economy.

Urban and regional I-O studies in the United States are usually designed to direct public policy (see Barnard 1969; Leven, Legler, and Shapiro 1970). Hirsch (1969) studied St. Louis's "health and well-being" by view-

ing the interindustry effects on employment stability, per capita income, and trade. He also applied I-O modeling to estimate the relationship between local industrial development and net changes in fiscal resources available to the area.

Input-output coefficients can be estimated either directly from a field survey or indirectly from (a) secondary data on industry receipts or employment, and (b) a related I-O model estimated from survey data. The latter approach, which we shall call "inferential I-O analysis," is less accurate but the former can be prodigiously expensive. The procedure based on secondary data is the one most often used at the national and interregional levels. Round (1978) argues that for large area analysis, the advantages of inferential I-O analysis clearly outweigh the disadvantages.

At the intraregional and urban levels the choice of techniques is not so clearly defined. Czamanski (1969) and Schaffer and Chu (1969) suggest tentatively that a survey should be taken. However, Goldman (1969) feels that the field survey is a waste of time and money. Miernyk (1969) qualifies other positions by arguing that national coefficients adjusted to a local level are of little use for the objective of long-range forecasts. Peterborough, England, is one of the rare cities in which the survey technique has been used to develop an I-O model (Morrison 1973). Morrison (1973) concludes, in one of the most nearly definitive works on the subject, that the inferential method is rather good, especially the "location quotient" method which is used in this paper.

Details of the computational procedures and methodology for the production of location quotients and complete regionalization of I-O tables are provided in Bureau of Economic Analysis (1977). Essentially, the procedure uses local data to see whether industry A, which is an input requirement of industry B, is present in the local area. If it is not, industry B in the local area must import industry A's product. The amount paid for that product is subtracted from the local interindustry transactions and added to the total imports. Location quotients express the proportion of industry B's input requirements that are present in the local area. The vector of location quotients represents the local area's production capacity relative to that of the larger area. In our case, the vector of location quotients for the Cuban community represents the production capacity of the Cuban community compared with that of Florida.

Specifically, the vector of location quotients is multiplied by the direct-input-requirements matrix computed from the known I-O table. The direct input requirements are computed by dividing each column transaction by the total column (industry) inputs in an interindustry transactions table. The result is an inferred-direct-requirements matrix for the local area. A direct-and-indirect-requirements matrix, which more thoroughly illustrates industrial interdependencies, is computed by subtracting the direct-

requirements matrix from an identity matrix and obtaining the inverse of the resulting matrix.²

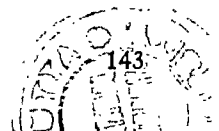
The accuracy of inferential I-O analysis depends on the accuracy of the assumptions made in building the location quotients (see Bureau of Economic Analysis 1977, pp. 104-5). In general, I-O analysis assesses the extent of interdependencies among industries in a given geographical area, and, conversely, the extent of interdependencies between local and nonlocal industries. Inferential I-O analysis differs from direct survey I-O analysis in that the inferential technique establishes the *maximum* amount of economic integration that is possible in an area given the area's production capacity. The degree of economic integration estimated from survey I-O analysis will always be equal to or less than the amount estimated from inferential I-O analysis. Theoretically, it is better to use direct survey I-O analysis, but from a practical standpoint the cost of direct survey I-O analysis, which has been as much as a million dollars for a single city (the cost varies depending on the size of the industry sample and the quality of interviewers who must assemble what is basically accounting information), has made inferential I-O analysis the more attractive alternative. One study (Morrison 1973) that compared the two techniques found that the inferential method produced fairly accurate estimates of interdependencies. Since the areas of analysis for enclave economies are smaller than the city economy examined by Morrison, and since ethnic economies may be subject to different influences from those of city economies, a direct survey of enclave interdependencies needs to be done in the future to assess more accurately the adequacy of the inferential method.

Since the inferential method is accurate for assessing the maximum amount of economic integration that is possible, the following discussion will be couched in these more conservative terms.

CUBAN AND BLACK INPUT-OUTPUT COMPARISONS

The production capacity of each community defines the structural limitation on the amount of industry integration in the Cuban and black commu-

² It is important to recognize that since the production capacities of both local communities are converted to location quotients, the comparative I-O analysis that follows essentially standardizes for differences in size between the two communities. Direct and direct-and-indirect-requirements matrices are also standardized in the same fashion. Standardizing allows us to test for differences in structure between the two communities while removing differences in size. However, the interrelationship between size and structure has been a serious issue for structural sociology, and even though we adjust for size, differences in size may still influence differences in structure or vice versa. A particularly important question for community development efforts is whether small business communities can achieve growth by structuring their communities in the image of the center economy. To the extent that they prove able to do so, smallness is not a handicap for a minority business community as long as there are enough businesses in the community to create this kind of structure.



nities, respectively. On one side of the coin, this information illustrates the community benefits of "perfect integration" (perfect given the production capacity). On the other, the severe limitations on the amount of possible integration created by the past history of industry investments are clearly defined. The best that certain industry configurations can yield may be very little.

Transaction Tables

Transaction tables (tables 1 and 2) describe the purchases and sales among all possible industry pairs (off-diagonal figures) and within each industry (diagonal figures). In each table, the processing sectors define the six industry categories for which there are available data for the community in question: construction, manufacturing, trade (including wholesale and retail trade), transportation and utilities, services, and other industries. Columns illustrate the maximal value of inputs that can be purchased from Cuban (table 1) or black (table 2) industries, and the value of the minimal amount that must be "imported" from outside the community. Rows illustrate the maximal value of outputs that can be sold to industries in the community (processing sector) and the value of the minimal amount sold to "final demand" (an aggregate of household consumption and investment, net exports, and government purchases of goods and services).

A comparison of the total outputs of Cuban and black industries reveals a discrepancy in the productive capacities of the two communities. Cuban construction receipts total over \$16 million, while black receipts total only a little over \$5 million. Manufacturing receipts in the Cuban community are over \$124 million, while black receipts are just under \$6 million. In the next two industrial categories, trade and transportation, Cuban receipts are more than double black receipts. In the two remaining categories, the former exceed the latter by about 50%.

The manner in which sales are broken down in the tables can be illustrated with the manufacturing industry. A maximum of \$4,181,600 of the total \$124 million Cuban manufacturing receipts is due to sales to Cuban construction firms, and another \$3,144,800 of manufacturing sales are to Cuban wholesale and retail trade firms (table 1). Table 2 shows that a maximum of \$226,690 of the total \$5,971,000 in black manufacturing receipts are from sales to black construction firms, and another \$282,270 of manufacturing sales are to black wholesale and retail trade firms.

The amount of "integration," or interdependence, among Cuban firms and among black firms is measured by the number and relative size of transactions among the processing industries of each minority. Quantitative measures of interdependence are calculated from the transaction tables. We will examine four measures of interdependence: the direct requirements

TABLE 1
TRANSACTION TABLE, CUBANS IN MIAMI
(Thousands of Dollars)

SALES									
PURCHASES	Processing Sectors					Final Demand			Total Output (Inter- industry and final demand)
	Construc- tion	Manufac- turing	Trade†	Transpor- tation	Services	All Other Industries	Total Processing or Interindustry Sales	Total Final Demand‡	
Processing sectors:									
Construction*	.00	.00	.00	.00	.00	.00	.00	16,463.00	16,463
Manufacturing...	4,181.60	43,505.99	3,144.80	245.31	895.27	821.22	52,794.19	71,864.81	124,659
Trade†	1,349.97	4,113.75	2,001.24	311.36	315.00	175.48	8,266.80	87,030.20	95,297
Transportation...	98.78	997.27	190.59	584.97	33.16	308.84	2,213.61	7,221.39	9,435
Services.....	82.32	997.27	2,096.53	94.35	348.16	49.13	3,667.76	12,911.24	16,579
All other industries.....	16.46	373.98	667.08	75.48	198.95	21.06	1,353.01	5,665.99	7,019
Subtotal.....	5,729.13	49,988.26	8,100.24	1,311.47	1,790.54	1,375.73	68,295.37	201,156.63	269,452
Final payments:									
Imports.....	3,720.64	21,316.69	18,392.32	2,660.67	5,189.23	814.20	52,093.75		
Value added†....	7,013.23	53,354.05	68,804.44	5,462.86	9,599.23	4,829.07	149,062.88		
Total inputs**	16,463	124,659	95,297	9,435	16,579	7,019	269,452		

SOURCES.—Transaction table regionalized from the Florida input-output survey; data from Florida Department of Commerce. Total output from U.S. Bureau of the Census, "Minority-owned Businesses—Spanish Origin," MB72-2, Special Report of the 1972 Survey (1974).

* All construction is considered investment and is sold to satisfy final demand, hence all row entries are zeros.

† Wholesale and retail trade margins.

‡ Value added includes compensation of employees, proprietors' income, rental income of personal, corporate profits and inventory valuation adjustment, net interest, business transfer payments, indirect business tax and non-tax liability. It excludes subsidies, current surplus of government enterprises, and capital consumption allowances.

§ Final demand includes personal consumption expenditures, gross private domestic investment, net exports, and government purchases.

** Total inputs equal total outputs for each industry.

TABLE 2

TRANSACTION TABLE, BLACKS IN MIAMI
(Thousands of Dollars)

PURCHASES	SALES					Final Demand	
	Processing Sectors					Total Processing or Interindustry Sales	Total Final Demand
	Construc- tion	Manufac- turing	Trade	Transpor- tation	Services	All Other Industries	
Processing sectors:							
Construction.....	.00	.00	.00	.00	.00	.00	5,152.00
Manufacturing....	226.69	358.26	282.27	9.14	89.29	1,059.11	4,911.89
Trade.....	437.92	197.04	987.95	75.44	188.50	2,003.68	45,041.32
Transportation....	25.76	41.80	94.09	123.44	19.84	177.57	1,803.50
Services.....	56.67	101.51	2,211.12	45.72	456.37	2,946.16	6,974.84
All other industries.....	10.30	47.77	752.72	43.43	287.71	32.71	3,498.36
Subtotal.....	757.34	746.38	4,328.15	297.17	1,041.71	7,666.09	67,381.91
Final payments:							75,048
Imports.....	2,199.90	2,669.04	8,750.37	665.23	3,135.04	962.64	18,382.22
Value added.....	2,194.76	2,555.58	33,966.48	1,323.60	5,744.25	3,215.02	48,999.69
Total inputs.....	5,152	5,971	47,045	2,286	9,921	4,673	75,048

Source.—Total output from U.S. Bureau of the Census, "Minority-owned Businesses—Black," MB72-2, Special Report of the 1972 Survey (1974).

tables, the direct plus indirect requirements table, the direct plus indirect employment requirements table, and a set of multipliers that summarizes each table.

Direct Requirements Tables

Direct input requirements per thousand dollars of gross output of Cuban and black industry in Miami are presented in table 3 along with inputs broken down by industrial category. The direct requirements table is computed by dividing cell values of the transaction tables by the column totals. Thus only the columns of the direct requirements table are interpreted. We have multiplied each resulting proportion by \$1,000 for convenience in interpretation. As shown in table 3, for every \$1,000 of construction output, Cuban construction firms buy \$254 of materials from Cuban manufacturing firms, \$82 from trade firms, \$6 from transportation firms, \$5 from services, and \$1 from all other Cuban industries. Black construction firms buy \$44 of materials from black manufacturing firms, \$85 from trade, \$5 from transportation, \$11 from services, and \$2 from all other black industries. Inputs to construction are similar in the two communities except for inputs from manufacturing. Cuban construction is able to purchase \$254 of its manufacturing requirements from Cuban firms while black construction must import \$210 of that amount, finding only \$44 of their manufacturing requirements in the black community and importing a total of \$328 from manufacturing firms. The reverse is true of the services requirements for construction. Cuban construction finds only \$5 of its services needs within the community, while black construction can input an additional \$6 of its services needs from black firms, making a total of \$11. The Cuban manufacturing advantage far outweighs this disadvantage in services. The net within-community advantage for Cuban construction inputs is \$201 (the difference between Cuban and black construction's total direct requirements in table 3).

The manufacturing inputs reflect a similar pattern. Cuban manufacturing derives \$349 of its needs from other Cuban manufacturing firms, while black manufacturing can only buy \$60 from other black manufacturing firms. Black manufacturing can purchase more services within the black community than can be purchased by Cubans in the Cuban community (\$17 vs. \$8), but again the manufacturing advantage far outweighs any disadvantages in other industries. The net total direct disadvantage for black manufacturing amounts to *additional* imports from outside the black community of \$276 for each \$1,000 of demand, that is, \$276 more than the Cuban community manufacturers must import or a total of \$447.

A similar pattern is found in the trade column, but this time the offsetting advantages and disadvantages more nearly balance each other.

TABLE 3

DIRECT REQUIREMENTS PER THOUSAND DOLLARS OF GROSS OUTPUT OF
COLUMN INDUSTRY PRODUCTS (in Dollars at Producers' Prices)

To Produce \$1,000 of Goods and Services by Column Industries Requires Direct Inputs from Row Industries in the Amount Shown	Construc- tion	Manufac- turing	Trade	Transpor- tation	Services	All Other Industries
Cubans in Miami:						
Cuban firms:						
Construction.....	\$000	\$000	\$000	\$000	\$000	\$000
Manufacturing.....	254	349	33	26	54	117
Trade.....	82	33	21	33	19	25
Transportation.....	6	8	2	62	2	44
Services.....	5	8	22	10	21	7
All other industries...	1	3	7	8	12	3
Total direct.....	\$348	\$401	\$ 85	\$139	\$108	\$196
Imports:						
Construction.....	\$000	\$000	\$000	\$000	\$000	\$000
Manufacturing.....	118	56	22	86	31	12
Trade.....	3	†	†	†	†	†
Transportation.....	17	16	6	33	6	18
Services.....	79	57	138	82	225	46
All other industries...	9	42	26	81	51	40
Total imports.....	\$226*	\$171	\$193	\$282	\$313	\$116
Blacks in Miami:						
Black firms:						
Construction.....	\$000	\$000	\$000	\$000	\$000	\$000
Manufacturing.....	44	60	6	4	9	20
Trade.....	85	33	21	33	19	25
Transportation.....	5	7	2	54	2	38
Services.....	11	17	47	20	46	16
All other industries...	2	8	16	19	29	7
Total direct.....	\$147	\$125	\$ 92	\$130	\$105	\$106
Imports:						
Construction.....	\$000	\$000	\$000	\$000	\$000	\$000
Manufacturing.....	328	345	49	108	76	109
Trade.....	†	†	†	†	†	†
Transportation.....	18	17	6	41	6	24
Services.....	73	48	114	72	200	37
All other industries...	8	37	17	70	34	36
Total import.....	\$427	\$447	\$186	\$291	\$316	\$206

NOTE.—Gross output generally represents the sum of industry production of primary, secondary, and transferred goods and services and substitutable imports.

* Purchase value inputs (column entries) may be read as standard coefficients by moving all decimals three places to the left.

† Less than \$.50.

Trade firms have a \$27 advantage with Cuban manufacturing (\$33 — \$6), but black services yield a \$25 advantage and other black industries yield a \$9 advantage. This offsetting pattern is repeated in the transportation and the services industry input requirements.

Finally, the pattern of inputs in the "other" category is similar to the pattern found in the requirements for construction and manufacturing. Cuban firms can buy \$117 of their input requirements from Cuban manufacturers while black firms can buy only \$20 of their direct requirements from black manufacturers. Black services offset only \$9 of the manufacturing disadvantage. Altogether, black "other" firms are required to import \$90 more than Cuban firms.

The pattern of direct requirements and imports in the two business communities is in agreement with our expectations. The different industry configurations in the two communities produce environments in which, in three of the industries (construction, manufacturing, and other), Cuban firms can buy many of their input requirements from Cuban-owned firms while black firms must buy predominantly from nonblack firms. In the other three industries (trade, transportation, and services), imports are roughly similar. There is little evidence of vertical integration in the black community; there is stronger evidence of it in the Cuban community.

Direct plus Indirect Requirements Tables

Indirect requirements are second, third, fourth, and higher rounds of spending carried out in order to meet the initial demand figure. Thus direct requirements will always equal demand (\$1,000 in our case), but direct plus indirect requirements will be higher than demand. This comes about because manufacturing, for example, must buy products from trade (and other industries) in order to satisfy the manufacturing input requirements of construction. Trade must in turn buy products from services (and others) in order to meet manufacturing's input requirements which were generated in order to satisfy construction's requirements. This process continues until all of the effects of the initial demand (\$1,000) have trickled out of the local economy.

Direct and indirect requirements per unit of final demand are generated by inverting the difference between the direct requirements matrix and an identity matrix. Essentially the direct plus indirect requirements tables repeat the pattern found in the direct requirements tables for smaller and smaller units of demand (specifically, those that occur in the cells of the direct requirements tables). Thus the pattern of interdependencies found in the direct plus indirect requirements table will be similar to those in the direct requirements tables. The magnitude of the difference between the two kinds of tables is due to the magnitude of the interdependencies among in-

dustries. An economy that is highly interdependent will produce a direct plus indirect requirements table that differs more from the direct requirements table than will an economy that is not highly interdependent.

Table 4 presents the direct plus indirect requirements table per \$1,000 of final demand for Cubans and blacks. Inputs produced solely by indirect requirements are in parentheses. We are most interested in these figures as they represent industrial interdependencies more purely than the others.

In the Cuban businesses, efforts to meet construction's direct input requirements create "ripples" of sales in the other industries which total to the amounts shown in parentheses in column 1. Indirect purchases are greatest in manufacturing, followed by trade which is much lower but still

TABLE 4
DIRECT PLUS INDIRECT REQUIREMENTS PER THOUSAND DOLLARS DELIVERY
TO FINAL CONSUMERS OF COLUMN INDUSTRY PRODUCTS
(in Dollars at Producers' Prices)

Direct and Indirect Requirements to Deliver \$1,000 of Goods and Services to Final Users by Column Industries Requires Direct and Indirect Inputs from Row Industries in the Amounts Shown	Construc- tion	Manufac- turing	Trade	Transpor- tation	Services	All Other Industries
Cubans in Miami:						
Construction.....	\$1,000	\$ 000	\$ 000	\$ 000	\$ 000	\$ 000
Manufacturing.....	396 (142)	1,542 (193)	55 (22)	47 (21)	89 (35)	185 (68)
Trade.....	98 (16)	53 (20)	1,024 (3)	38 (5)	23 (4)	34 (9)
Transportation.....	10 (4)	14 (6)	4 (2)	1,067 (5)	4 (2)	49 (5)
Services.....	11 (6)	14 (6)	23 (1)	12 (2)	1,023 (2)	10 (3)
All other industries.....	3 (2)	6 (3)	7 (0)	9 (1)	13 (1)	1,005 (2)
Total inputs.....	\$1,518	\$1,629	\$1,113	\$1,173	\$1,152	\$1,283
Blacks in Miami:						
Construction.....	\$1,000	\$ 000	\$ 000	\$ 000	\$ 000	\$ 000
Manufacturing.....	47 (3)	1,065 (5)	7 (1)	6 (2)	11 (2)	22 (2)
Trade.....	89 (4)	37 (4)	1,023 (2)	37 (4)	22 (3)	28 (3)
Transportation.....	6 (1)	9 (2)	3 (1)	1,058 (4)	4 (2)	41 (3)
Services.....	17 (6)	21 (4)	51 (4)	25 (5)	1,050 (4)	20 (4)
All other industries.....	5 (3)	10 (2)	18 (2)	22 (1)	31 (2)	1,010 (3)
Total inputs.....	\$1,164	\$1,142	\$1,102	\$1,148	\$1,118	\$1,121

NOTE.—Indirect requirements are in parentheses.

in double digits. Cuban manufacturing produces indirect purchases from manufacturing which are even greater than those produced by construction requirements. Cuban manufacturing produces indirect purchases from trade which are also in double digits (\$20). The other four Cuban industries (trade, transportation, services, and other) do not produce nearly so much in indirect purchases (cols. 3-6). Only those from manufacturing reach double digits. All other indirect requirements are less than \$10. The indirect requirements in the black community are uniformly low. Only one indirect requirement reaches as high as \$6 per \$1,000 of delivery.

These results bring the results from the direct requirements tables into sharper relief. The interdependencies generated by Cuban manufacturing, and to a lesser extent construction, now begin to dominate the Cuban-black comparisons. Furthermore, these interdependencies in Cuban industry extend partially to trade. The indirect requirements of Cuban construction and manufacturing for trade both reach double digits. Also, it becomes clear that the rest of the Cuban economy is poorly integrated, with the exception of its integration with manufacturing. This particular form of economic structure will be considered in more detail in the concluding section of the paper.

Industry and Employment Multipliers

Type I industry multipliers provide a useful summary of the preceding tables. They are the ratio of direct plus indirect requirements to the total direct requirements for each industry, and they represent the total amount of expenditures generated in the community per dollar of final demand. The total direct requirements are \$1,000 so this means simply shifting the decimal point three places to the right for the panel totals in table 4. They are presented in the first and second columns of table 5.

Also in table 5 are the Type I employment multipliers constructed from the direct employment requirements in columns 3 and 4 of table 5 and the direct plus indirect employment requirements presented in table 6 and totaled by industry in the fifth and sixth columns of table 5.

The black direct labor requirements per unit of demand (table 5, col. 4) are uniformly higher than those of Cuban industries (col. 3). For every \$1 million of final demand, black construction firms hire nine more employees than Cuban construction firms (since the figures in table 6 and in col. 3 through 6 of table 5 are expressed in man-years per \$1,000 of final demand, multiply each figure by 1,000 to get man-years per \$1 million of demand). Black manufacturing firms use 21 more employees than Cuban manufacturing. Black trade uses 3 more employees, transportation 13 more (almost double), services 7 more, and "other" industries 10 more employees per \$1 million of final demand than Cuban firms.

TABLE 5
ESTIMATED MULTIPLIERS FROM THE CUBAN AND BLACK INPUT-OUTPUT AND EMPLOYMENT TABLES

	TYPE I INDUSTRY MULTIPLIERS		CHANGES IN DIRECT EMPLOYMENT		CHANGES IN DIRECT AND INDIRECT EMPLOYMENT		TYPE I EMPLOYMENT MULTIPLIERS	
	Cuban (1)	Black (2)	Cuban (3)	Black (4)	Cuban (5)	Black (6)	Cuban (7)	Black (8)
Construction.....	1.52	1.16	.036	.045	.052	.050	1.44	1.11
Manufacturing.....	1.63	1.14	.035	.056	.056	.061	1.60	1.09
Trade.....	1.11	1.10	.014	.017	.018	.021	1.29	1.24
Transportation.....	1.17	1.15	.014	.027	.018	.032	1.29	1.19
Services.....	1.15	1.12	.049	.056	.054	.061	1.10	1.09
All other industries.....	1.28	1.12	.029	.039	.037	.043	1.28	1.10

TABLE 6
DIRECT AND INDIRECT EMPLOYMENT REQUIREMENTS IN MAN-YEARS
PER THOUSAND DOLLARS OF FINAL DEMAND

For Every \$1,000 of Final Sales Made by Column Industries, Direct and Indirect Man-Years of Labor in the Tabulated Amounts Will Be Required from These Labor-supplying Row Industries	Construc- tion	Manufac- turing	Trade	Transpor- tation	Services	All other Industries
Cubans in Miami:						
Construction.....	.03572	.00000	.00000	.00000	.00000	.00000
Manufacturing.....	.01382	.05380	.00190	.00165	.00310	.00646
Trade.....	.00137	.00074	.01436	.00053	.00033	.00047
Transportation.....	.00014	.00020	.00005	.01338	.00005	.00070
Services.....	.00053	.00066	.00115	.00057	.04998	.00050
All other industries.....	.00010	.00018	.00021	.00027	.00038	.02848
Total.....	.05168	.05558	.01767	.01840	.05384	.03661
Blacks in Miami:						
Construction.....	.04445	.00000	.00000	.00000	.00000	.00000
Manufacturing.....	.00259	.05864	.00038	.00033	.00062	.00122
Trade.....	.00144	.00059	.01649	.00059	.00035	.00046
Transportation.....	.00016	.00023	.00009	.02869	.00010	.00111
Service.....	.00097	.00115	.00284	.00139	.05861	.00109
All other industries.....	.00020	.00042	.00074	.00090	.00129	.03934
Total.....	.04980	.06103	.02054	.03190	.06097	.04322

Still, almost all of the Cuban-employment multipliers (col. 7) are higher than the black-employment multipliers (col. 8). Construction, manufacturing, trade, and transportation produce higher employment multipliers in Cuban-owned businesses. The Cuban multiplier in construction is 33% higher than the black multiplier. The Cuban multiplier of 1.44 means that unit demand in construction (the amount of demand that it takes to create one job) creates nearly an additional one-half job (actually, 0.44 of a job) owing exclusively to interdependencies. Unit demand produces 1.60 jobs in Cuban manufacturing, and the amount of demand that it takes to produce one job in each of trade and transportation produces an additional 0.29 of work because of interdependencies from indirect requirements.

Thus, although black industries hire more workers than Cuban industries, increases in demand produce more jobs in the Cuban community than in the black community.

SUMMARY AND CONCLUSIONS

The purpose of this paper was to explore the relationship between the economic structure of ethnic enclaves and the overall economic well-being of the enclave. Specifically, we wished to investigate the presence of core or periphery-style economic structuring; the former was expected to be present in the more developed Cuban enclave in Miami, and the latter was expected to be present in the less developed black enclave in Miami. We used inferential techniques to examine the amount of vertical integration, or interdependencies, among Cuban-owned businesses and among black-owned businesses. However, data limitations forced us to moderate our goals. The analysis investigates the maximum amount of vertical integration that is possible within each minority-owned business community given its production capacity.

Results indicate that the potentials for vertical integration are substantially higher among Cuban industries than among black industries. Although the actual amount of vertical integration is probably lower than the theoretical maximum, the important finding is that the configuration of production capacities in Cuban industries is such that a high degree of vertical integration is possible. In some instances, the amount of vertical integration can create additional spending of over 50% within the community after the injection of initial demand. In the black community, even the *potential* for vertical integration is absent. If the actual black interdependencies reach the theoretical maximum, which is not probable, the greatest amount of additional spending that could be created in the black community, after the injection of initial demand, would be 16%.⁸

⁸ It should be kept in mind that the improbable sometimes turns out to be true. Since we employ an inferential I-O technique, it is logically possible that if blacks are at

The black business community appears to be merely an extension of the periphery economy in America even though the community appears to be somewhat distinct by virtue of the fact that the businesses are minority owned and somewhat geographically isolated. The Cuban community, on the other hand, seems to have created a genuine alternative to the dual economy. This third alternative is an enclave economy characterized by a greater degree of autonomy from the majority economy and a greater degree of self-enclosed interdependence than is characteristic of the black community. The three structural alternatives can be summarized as follows: (1) center economy, composed of large monopolistic firms which are *individually* vertically and horizontally integrated (majority only); (2) periphery economy, composed of many small "atomized" businesses (mixture of majority- and minority-owned firms including those owned by the black community in Miami); and (3) enclave economy, composed of clusters of small businesses which are *collectively* vertically and horizontally integrated (usually minority-owned businesses such as those owned by the Cuban community in Miami).

The inferential I-O technique seems to be a revealing method for uncovering differences in the economic structures of the two communities. We believe that inferential I-O analysis is a useful addition to the repertory of tools for the analysis of problems in race and ethnicity. Still, caution is advisable in the use of the technique until direct-survey I-O analysis has been used to check the accuracy of the inferential technique. Although such checks have been performed at the city level (Morrison 1973), the complex dynamics of intercommunity relationships, especially among communities of different ethnicity, may alter interindustry flows significantly from those expected on the basis of the inferential method.

While the main purposes of this paper have been to establish the worth of structural notions of enclave economies and to explore the advantages and disadvantages of inferential I-O analysis, the larger sociological interest lies in carrying the question further: Why has the Cuban enclave achieved a working form of center-economy organization while the black enclave has not? Since our work does not bear directly on this question we cannot provide a definitive answer, but there are several fairly obvious differences in the origins of the two communities that may provide a satisfactory explanation.

Light (1980) has distinguished between two sets of influences from the their maximum (a manufacturing multiplier of 1.14) and the Cuban community is far below its maximum (let us say a multiplier of 1.05 instead of the estimated value, 1.63), then our conclusions would be in error. Intuitively this does not seem reasonable and inferential comparisons with direct survey estimates at the city level indicate that inferential estimates are reliable. Still, until direct surveys are conducted at the community level there is no way to gauge accurately the reasonableness of the possibility that black community businesses are more interdependent than Cuban ones.

origins of the ethnic group, "orthodox" or cultural/historical influences and "reactive" or more immediate situational influences, which were important in the growth of Korean business in Los Angeles. Both sets of influences seem to distinguish Cuban and black communities in Miami. Perhaps the most important factor is situational. Miami is strategically located for international trade, and the Spanish-speaking business population in the Cuban community has been at the forefront in the development of trade with Latin America, the Caribbean, and most recently with Europe (Strategy Research Corporation 1978). Much-improved communication and transportation facilities between Miami and Latin America and the establishment of large branch offices of New England banks in Miami have made Miami an attractive center for trade among nation-states in that part of the globe.

Even though there may be mutual antagonisms and jealousies between the two communities, growth in the Cuban community has not been a process of taking over or driving out black competition, with the exception of the meat industry (Luytjes and Yuhasz 1979). The Cuban community has obtained its success primarily from the production of ethnic goods, textiles, cigars, and food, and from the Latin connection, the development of which coincides with the growth phase of the Cuban enclave.

Cultural factors which seem to favor the Cuban community are the greater experience of the Cuban immigrants with an entrepreneurial economy and the greater access to investment capital enjoyed by the Cuban community. Earlier Cuban immigrants escaped Cuba with a little capital (even though Castro imposed restrictions in an attempt to prevent the removal of capital). Later immigrants were able to save money during the government's job dispersal program and to return to Miami with some capital for investment (Wilson and Portes 1980). Also, local banks may have been more willing to make loans to Cuban businesses, especially those with the potential for international trade.

In various combinations, these sets of influences created a monopolistic environment resulting in center-economy organization. While the initial impetus for the migration from Cuba was political (the desire to escape Castro's Cuba), the migration filled the need for a viable Spanish-speaking economy located on the coast of the United States which would help promote the new international economy of which Miami was the hub.

While this sheds light on the reasons for the unique success of the Cuban enclave, it still does not explain the lack of center-firm organization in the black community. Although the availability of capital is not sufficient to guarantee the growth of center-economy organization, it is clearly necessary for the growth of any kind of economy. Luytjes and Yuhasz (1979) polled black businessmen and black community leaders in Miami. They listed the lack of available capital as a primary reason for the problems of the

black business community. Furthermore, it was noted that even when capital was made available, technical assistance for proper investment strategies was inadequate. Bates and Bradford (1979) have pointed out that availability of capital to inner-city black areas may continue to be inadequate, partly because of the new social consciousness of the plight of blacks. Banks may consider such loans too risky if public sympathy for the black situation prevents foreclosure or other legal action to recover losses. Finally, the unavailability of capital may directly discourage center-economy organization by preventing competition for public contracts. In Luytjes and Yuhasz's (1979) survey it was noted that black small business owners lack the money necessary for bonding and therefore cannot afford to bid on large public contracts.

Another significant reason for the plight of the black community is the absence of an exploitable labor force. Most of the labor in the black community is exported to white business. Black workers forgo the possible long-term benefits of employment in black businesses for short-term gains in higher wages in white business, even though these wages may be relatively lower than white wages and earned in jobs with little chance for promotion (Harrison 1974). More highly trained blacks, who could be promising entrepreneurs, seem to prefer jobs with government agencies (Luytjes and Yuhasz 1979). As a result, the political power of the black community often bypasses the black businessmen. Most of the representatives of the community are either workers or ministers.

Thus, historical factors in a black enclave, difficulties in obtaining viable capital, and situational factors creating other employment alternatives have channeled the black labor force into employment in white industries. It is these blacks who find greater political representation. The structured interdependence with the white community is also evident in the I-O tables for the black community. Black business shows a greater reliance on nonblack inputs. Also, black consumers seem to prefer nonblack suppliers. This may be the result of adverse business conditions in the black community. For example, prices in black businesses are set high in order to offset losses from pilferage (Luytjes and Yuhasz 1979).

These and other factors need to be examined in the future. In general, the theory of self-enclosed ethnic enclaves can benefit from positing a notion of the economic structure of the enclave as an intervening concept between traditional sociological influences and the general condition of the enclave. As work progresses, we can begin to understand the specific role of each factor in the historical process of enclave development. At this point we can only list probable causes. We must rely on future work to establish more systematic interlinkages between specific "reactive" and "orthodox" influences and the particular business patterns they seem to engender.

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Commentary and Debate

To conserve space for the publication of original contributions to scholarship, the comments in this section must be limited to brief critiques. They are expected to address specific errors or flaws in articles and reviews published in the *AJS*. Comments on articles are not to exceed 1,500 words, those on reviews 750 words. Longer or less narrowly focused critiques should be submitted as articles. Authors of articles and reviews are invited to reply to comments, keeping their replies to the length of the specific comment. The *AJS* does not publish commenters' rebuttals to authors' replies. We reserve the right to reject inappropriate or excessively minor comments.

DETERRENCE AND THE DEATH PENALTY: A COMMENT ON PHILLIPS

In "The Deterrent Effect of Capital Punishment" (*AJS* 86 [July 1980]: 139-48), Phillips modestly claims to contribute "the first compelling statistical evidence that capital punishment does deter homicides for a short time" (p. 139). The basic idea seems sensible enough; most of us have observed how freeway traffic slows down on approaching the site where a motorist is receiving a traffic ticket, but speeds up once the incident is safely out of view. Phillips contends that much the same thing happens with the relationship between executions and homicide: a short-term deterrent effect is followed by a long-term increase in the deviance. However, his supporting evidence is hardly "compelling." We consider the following basic issues: (1) arbitrary time lags; (2) an ambiguous independent variable; and (3) lack of controls.

The point of departure for Phillips's analysis is the observation that "researchers may have failed to discover a deterrent effect because almost all studies on capital punishment use *yearly* rather than weekly or daily homicide data" (p. 140). At the outset, Phillips appears to be presenting a study of lags between execution and homicide. Rather than make use of his data to study lags, however, he capriciously seizes on "a week" as the relevant unit and divides his sample of homicides according to weeks preceding, following, and concurrent with the date of publication of an execution.

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Phillips claims to present "weekly homicide statistics for a 57-year period" (p. 140). Actually he presents three weeks of homicide data around each of 22 executions over a 57-year period, or a total of 66 weeks of homicide data. (Parenthetically, he provides an analysis of nine weeks of data on the 22 executions, or 198 weeks of data.) A substantial external validity problem is involved in generalizing to capital punishment from these 22 incidents. More likely, he could at most generalize to "heavily publicized" executions of "notorious murderers."

His sole justification for the inference of a deterrent effect of capital punishment is a supposed significant drop in the number of homicides during the experimental period. His "experimental period" is the week when the execution was publicized in the newspaper. The control period is the two weeks straddling the experimental period. Phillips computed the average weekly number of homicides for the control period, and subtracted this value from the number of homicides during the experimental period. A negative result indicated a decrease in the number of homicides. He ignores three cases in which there was no change, and compares 15 negative results with four positive results over the 22 incidents.

Several aspects of this test are troubling. First, Phillips gives no justification for ignoring the three cases lacking change, although it is clear that ignoring them provides the only ground for applying a binomial test. However, "no change" could be interpreted as "no deterrence." This point would not reverse his findings, but does have implications for further analyses using the techniques he proposes.

Second, a total of 19 homicides are reported as occurring during the experimental week, an average of 0.86 per execution incident. One incident had six homicides, while four incidents had two homicides during the experimental week. Twelve incidents had no homicides during this week. With only 22 incidents, and 12 zeros, significance under Phillips's binomial test is all but assured, regardless of the week or average of weeks used for comparison. The structure of this test hardly appears reasonable.¹

In interpreting his findings Phillips appears to be confused over just what his experimental period represents. He states: "Homicides drop significantly in the week of a publicized execution" (p. 144); if we are to believe his actual test (we do not), he, in fact, finds that homicides drop significantly in the week of *publication* of an execution. He presents no data to indicate that the executions occurred during the experimental week. In fact, date of execution seems to bear no relationship whatever to date of publication of execution, yet Phillips refers to his experimental week as "the week of the execution" and the "execution week."

¹ Phillips assures us that his test is insensitive to the definition of the control period (p. 143). This insensitivity appears to be completely attributable to the 12 zeros.

Table 1 gives the frequency of day of week for the 22 publications of executions. Almost two-thirds of execution publications appeared on Wednesday. Given that the executions themselves occurred more or less randomly across days of the week, it is curious that so many should be published on Wednesday. One might wonder whether Phillips's independent variable might more accurately be depicted as the publicity about execution rather than the actual springing of the trap door itself. He appears unable to separate effects of capital punishment as an act itself from the publicity about the legal processing of capital crimes (Lauderdale 1980). Further, this calls into question his attribution of the "drop" in homicides to a short-term effect of an instance of capital punishment. He claims that the effect is of one week's duration. Of the 22 executions, 86% occurred at mid-week or after, leaving at most three days for deterrent effects to reduce homicides.²

What Phillips has actually tested is the deterrent effect of publication of capital punishment, plus whatever residual effects of capital punishment might remain (those unconfounded with the control period). Even this limited test is misleading because the number of column inches in the *Times* of London does not refer simply to the day or even week of execution but, instead, typically to stories concerning a variety of events surrounding the proposed execution that extend over months of publication. For example, the case concerning Dr. Crippen appeared in the newspaper over a number of months. Much of the journalism entailed reports noting that petitions were being received urging the state to stay the execution, reports focusing on legal details of the case, and so on, and, in fact, on the day of the execu-

TABLE 1
DAY OF WEEK BY PUBLICATION
OF EXECUTION

Day	Frequency	Percentage	Cumulative Percentage
Sunday.....	0	.0	.0
Monday.....	1	4.5	4.5
Tuesday.....	2	9.1	13.6
Wednesday....	14	63.6	77.2
Thursday.....	2	9.1	86.3
Friday.....	1	4.5	90.8
Saturday.....	2	9.1	99.9
Total.....	22	99.9	...

² Phillips also claims that the more column inches are devoted to an execution story, the greater will be the deterrent effect. This claim is based on a Spearman rank-order correlation coefficient computed between ranked length of the story in the *Times* and the difference in murders between the experimental and control periods (p. 142, col. 7 of table 1). Obviously, there are far too many ties in col. 7 to compute a meaningful Spearman coefficient (there are eight values of $-.5$ alone).

tion there was only one small paragraph stating that the execution was to have occurred that morning.

In the light of recent advances in deterrence research (summarized in Blumstein et al. [1978]; see particularly Ehrlich [1973, 1975, 1977]) of the past decade, Phillips's methodology simply does not measure up.⁸ This research tradition has emphasized the need for (1) controlling for potential common causes (to guard against spuriousness);⁴ (2) assessing the impact of measurement error on observed relationships; (3) separating incapacitative and deterrent effects; and (4) estimating simultaneous models of the relationships between crimes and sanctions. Phillips addresses none of these critical problems.

Finally, part of our motivation for commenting on a short research note investigating deterrent effects and capital punishment should be emphasized. The issue of deterrence is of more than scholarly concern (e.g., the United Press International picked up the study and broadcast it as scientific evidence that capital punishment deters homicide). In addition, results reported in such social science journals as the *AJS* are routinely incorporated into court decisions in this area (e.g., *Furman vs. Georgia*, 1972). In short, we recognize the gravity of Phillips's closing discussion of policy implications and suggest a critical review of his research on the death penalty. This comment is the first step in that procedure.

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⁸ Phillips argues that his methodology is unique and implies that the bulk of the deterrence literature is not relevant to his work. We are obviously not convinced.

⁴ Phillips apparently assumes that spuriousness could not operate, since his time periods are short and close together. However, this assumption is precarious; he does not mention less control, extraneous events, or processes that might be responsible for decreases in the number of homicides.

THE FLUCTUATION OF HOMICIDES AFTER PUBLICIZED
EXECUTIONS: REPLY TO KOBBERVIG, INVERARITY,
AND LAUDERDALE

It is gratifying that my paper has attracted attention from such a wide range of researchers, from the honored Hans Zeisel, who has emerged from retirement to comment on my paper, to Kobbervig, Inverarity, and Lauderdale, who have just begun their research careers. Let us consider in turn the points addressed by Kobbervig and his colleagues.

Use of the week as unit of analysis.—Kobbervig, Inverarity, and Lauderdale comment that “[Phillips] capriciously seizes on ‘a week’ as the relevant unit” of analysis. Far from acting capriciously, I laboriously discovered that the week was the only available unit of analysis. As I noted in my paper, I searched “the vital statistics collections of the Library of Congress, the National Library of Medicine, and the British Museum” (p. 140) for a published set of daily or weekly homicide statistics in a country practicing capital punishment. The only useful data I found were weekly homicide figures published by Great Britain’s General Register Office. Thus, the weekly unit of analysis was determined by the available data.

Relationship between day of execution and day of story about execution.—Kobbervig et al. state that “almost two-thirds of execution publications appeared on Wednesday. Given that the executions themselves occurred more or less randomly across days of the week, it is curious that so many should be published on Wednesday.” The assumption that executions occurred “more or less randomly across days of the week” is erroneous. In fact, the English authorities markedly preferred Tuesdays for executions during the period I studied. Thus, the reason that so many English executions were reported on Wednesday is that so many English executions occurred on Tuesday.

Misuse of the Spearman Rank Correlation Coefficient.—Kobbervig et al. have noted that “Phillips also claims that the more column inches are devoted to an execution story, the greater will be the deterrent effect. This is based on a Spearman rank-order correlation coefficient.” (See their footnote 2.) Kobbervig et al. dismiss this finding in a footnote because “obviously, there are far too many ties . . . to compute a meaningful Spearman coefficient” (n. 2). Kobbervig et al. are in error. In my paper I stated that the Spearman rank correlation coefficient I calculated was “corrected for ties” (p. 144). Evidently, Kobbervig et al. did not notice this statement. (For a discussion of this standard correction, see Siegel [1956].)

Misuse of the binomial test.—Kobbervig et al. maintain that I have misused the binomial test in such a way as to increase the chances of rejecting the null hypothesis. They note that there are a large number of experimental weeks in which no homicides occur, and they feel that this circum-

stance unfairly increases the chance of rejecting the null hypothesis. This argument would have some merit if one first observed the data and *then* designed a test of the null hypothesis. However, the argument has no merit if one first designs the statistical test (as I did) and then observes the data. Under the latter circumstances, the binomial is an appropriate and unbiased test of significance.

The use of alternative methods to analyze the data.—Kobbervig et al. are dissatisfied with my use of simple nonparametric tests and prefer that I use the more complex techniques of some other researchers (“particularly Ehrlich [1973, 1975, 1977]”). It is strange that they choose to cite Ehrlich as a model researcher, because the techniques he employed have been so vigorously and destructively criticized. Zeisel’s review article devotes nine pages (1976, pp. 329–37) to summarizing the criticisms directed by other researchers at Ehrlich’s methodology (e.g., Bowers and Pierce 1975; Passell 1975). A more recent and highly regarded review of the literature (Blumstein, Cohen, and Nagin 1978) also presents very extensive criticisms of Ehrlich’s techniques.

On the other hand, my methodology has stood up under careful scrutiny by journals with the highest critical standards (*Science*, the *American Sociological Review*, and the *American Journal of Sociology*). The techniques employed in my analysis of homicides have been used repeatedly to analyze short-term fluctuations in various types of mortality (e.g., Phillips, 1974, 1977, 1978, 1979, 1980, 1981, 1982), and the methodology in these papers has not been seriously questioned.

Two points of agreement with Kobbervig et al.—Kobbervig et al. have noted that I measured publicity associated not only with the execution but also with the crime and trial that preceded it. This is correct. As I indicated in my paper, “Researchers have argued that a publicized execution should have its greatest effect if the murder, the trial, and the execution are all heavily publicized and occur within a short space of time. Consequently, the publicity devoted to an execution story has been defined as publicity devoted to the crime, the murderer, his trial, and his execution” (p. 143, n. 9).

Finally, Kobbervig et al. have noted that “the issue of deterrence is of more than scholarly concern.” I strongly agree, and I would like to take this opportunity to repeat a statement I made in the paper in question. “It is important, even urgent, to determine whether the findings I have reported can be replicated with American data. . . . These studies may shed some light on a scientific field which has been marked by 100 years of inconclusive results and a great deal of controversy. In addition, these studies have the potential to influence a key issue in American public policy—the debate over capital punishment” (p. 147).

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A COMMENT ON "THE DETERRENT EFFECT OF CAPITAL PUNISHMENT" BY PHILLIPS

Scholarly papers that claim to have found evidence of a deterrent effect of the death penalty quickly find their way into the general media and from there to the prosecutors and the arguments they make in courts, thus acquiring an importance far transcending the normal attention paid to academic endeavors. Any such claim, therefore, deserves scrutiny.

The latest such claim is made by David P. Phillips in his paper "The Deterrent Effect of Capital Punishment" (*AJS* 86 [July 1980]: 139-48). There he reports his analysis of the effect of 22 publicized executions that took place in England between 1858 and 1921. The effect he found is succinctly described in figure 1 (p. 145). It is summarized by the statement that "homicides are temporarily deterred . . . ; then the temporarily deterred homicides reappear" (p. 145). Phillips believes this "to be the first compelling statistical evidence that capital punishment does deter homicides for a short time" (p. 139). Compelling or not, two earlier studies of the problem deserved better than Phillips's footnote 5 that dismisses them as "inconclusive" (p. 140). Savitz (1968) tried to find an effect of executions on

the homicide rate in Philadelphia and failed to find one; "inconclusive" is a biased word for such a result. Graves's (1956) 1946-55 California study deserved better for another reason: His data, properly analyzed, foreshadow the possibility of Phillips's London findings.

Graves counted the number of homicides in San Francisco for the days prior, during, and following executions. His analysis of the data is somewhat off, but if the week following the execution date is compared with the seven-day period before executions, the curve shown in figure 1 is obtained. In a 1976 paper, "The Deterrent Effect of the Death Penalty: Facts v. Faith," I interpreted the data as follows: "The results would then suggest a reduction of homicides during the first three days following executions compensated by an increase during the rest of the week" (p. 328).

I wonder whether the relationship found by Phillips warrants even the word deterrence. The more adequate word would have been "delay." Suppose the curve of bank robberies after spectacular arrests of bank robbers showed a similar shape; would we say more than that some bank robbers delayed their plans for a couple of weeks?

Last, but most important, the controversy over the death penalty is over the narrow issue whether the death penalty deters homicides more effectively than life imprisonment. And to this, the real question at issue, Phillips's study is simply irrelevant, as would be any study that fails to compare the effects of executions with the deterrent effect of sentences to life imprisonment. I might add, in passing, that realistic potential candidates for execution, such as Heirens, Speck, Manson, or the Son of Sam, invariably receive real life sentences, not only nominal ones.

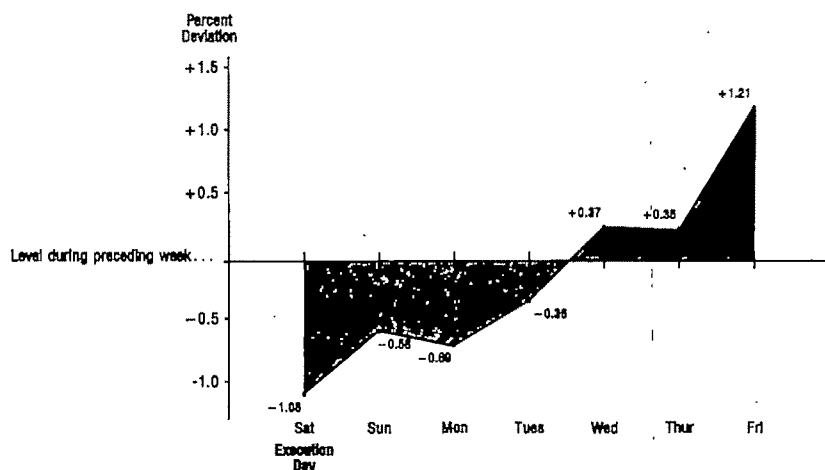


FIG. 1.—Homicides during the seven days following an execution compared with the level in weeks prior to executions (California counties: Alameda, Los Angeles, and San Francisco, 1946-55). (Source of data: Graves 1956.)

There are, moreover, somewhat disquieting aspects of the selection procedure of the 22 cases that form the data base for Phillips's study. David A. Thomas of the Institute for Criminology at the University of Cambridge drew my attention to the fact that merely during the years 1900-1921 there were 303 executions in England and Wales, of which exactly eight are in Phillips's study. To be sure, he was interested only in "heavily publicized" cases. But his basic measure of that qualification was the number of column inches devoted to a case by the *Times* of London, a newspaper that is not known for catering to the criminal classes. These inches, I might note, do not refer to the executions but to the total murder story, including the trial. Thus, the "inches in the *Times*" have a twofold index function to bear: the total publicity of the case is an index of the attention paid to the execution, and the reporting in the *Times* is an indication of the publicity elsewhere.

Within these limitations, Professor Phillips then reported two further discretionary choices. The 22 cases were culled from "a standard casebook of notorious murderers" (p. 140) (Wilson and Pitman 1962). Apparently, there was some choice as to which of several books to choose. In a footnote Phillips reassures us: "This volume [Wilson & Pitman] was used in preference to alternative [less comprehensive] sources." In that footnote he also advises us: "I omitted publicized executions during World War I and the Boer War because war news overshadowed other types of news during these periods" (p. 141, n. 6). We are not told how many qualified cases were omitted. Thus, there was perhaps altogether too much unfettered discretion in the selection of 22 cases from at least a thousand executions during the time period from 1858 to 1921.

To end on a conciliatory note in the case of Phillips's study, abolitionists may be willing to waive the essential requirement for controlled research and settle for Phillips's claim that his finding, that executions may delay some homicides for a short while, is "the first compelling statistical evidence" that capital punishment does anything.

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DETERRENCE AND THE DEATH PENALTY: REPLY TO ZEISEL

I appreciate Zeisel's close attention to my paper and I am happy to respond to his comments. He is disturbed that I labeled the work of Savitz and Graves "inconclusive." Let us see whether this label is appropriate.

Savitz's study.—Savitz himself considered his investigation to be inconclusive, for he noted that "the numbers involved are minute and are not susceptible to any rigorous statistical manipulation" (1958, p. 341). Furthermore, he noted, "It can be said in summary that the short period of time under analysis and the extremely small number of murders dealt with prevent conclusive findings" (1958, p. 341). Savitz's results are based on a small sample, are not tested for statistical significance, and are not corrected for trend. Under these circumstances, it seems reasonable to consider them inconclusive.

In addition, Zeisel states that "Savitz tried to find an effect of executions on the homicide rate." Unfortunately, Zeisel is in error: Savitz studied the effect of death sentences, not the effect of actual executions. Indeed, of the four death sentences studied by Savitz, only two ended in executions. Zeisel's regrettable confusion of the death sentence with the execution also appears in his 1976 review article (cited in his comment).

Graves's study.—We have seen that Savitz's study is inconclusive because the results are not corrected for trend, are not tested for significance, and are based on a very small sample. Despite these defects, Zeisel believes that Savitz's study is *superior* to Graves's. In his review article, Zeisel noted that "the failure of [Savitz's] more sensitive Philadelphia data to show any effect casts doubt on the strength of [Graves's] California result" (1976, p. 328). If Savitz's study is inconclusive and is, nonetheless, superior to Graves's, it seems reasonable to consider Graves's study inconclusive also.

Whether or not one agrees with Zeisel's assessment of the relative merits of Savitz versus Graves, there is independent evidence that Graves's results are inconclusive. Graves did not demonstrate that his results are statistically significant.¹ Results of this nature are generally considered to be inconclusive by nearly all researchers.

Zeisel's reanalysis of Graves's data.—In an effort to improve on Graves's study, Zeisel has reanalyzed the data. Graves compared the number of homicides in the execution week with the number of homicides in two types of control period. The first type was the week before the execution

¹ Graves did assert that his results were "probably significant. There is less than one chance in twenty that such a variation is due to chance alone" (1967, p. 327). But he did not indicate how he reached this conclusion. He did not name the statistical test he used; nor did he indicate whether it was one-tailed or two-tailed, etc. In its present form, Graves's attribution of statistical significance to his findings must be considered an assertion without evidence, rather than a demonstration.

week, while the second was the week after. By choosing control periods which straddled the execution week, Graves was able to correct for the effect of time trends and seasons on his homicide data.

Zeisel has chosen to reanalyze Graves's data by arbitrarily omitting the second type of control period from the examination. Zeisel compares the number of homicides in the execution week with the number of homicides in the week before. Proceeding in this fashion, he can no longer correct for the effect of time trends on the data. If one now finds a difference between the number of homicides in the execution week and the number of homicides in the week before, one cannot determine how much of this difference results from trends over time and how much results from the effect of the execution.

In sum, the reanalysis of Graves's data does not seem to be an improvement, because it arbitrarily throws away data and fails to correct for the effect of time trends. Incidentally, Zeisel does not demonstrate the statistical significance of his "findings." This is another reason for doubting that he has improved on Graves's analysis.

Procedure for selecting executions to be studied.—Zeisel is disturbed that I examined the effect of 22 highly publicized executions, rather than the effect of all executions. I proceeded in this fashion because I assumed that publicized executions would have a larger effect than unpublicized ones. Having made this assumption, I then searched for the most comprehensive possible list of notorious murderers and examined the effect of their executions. In my paper, I named alternative texts giving lists of notorious murderers and indicated that these texts contained fewer cases than the one I used.

My assumption that publicized executions have a greater effect than unpublicized ones is supported by my data. I found that the amount of newspaper space devoted to the execution story was significantly correlated with the drop in homicides after the execution—the greater the publicity given to the execution story, the greater the drop in homicides thereafter. This finding (which Zeisel has chosen to ignore) tends to increase one's confidence in the other findings I reported in the paper.

Two points of agreement with Zeisel.—Zeisel regrets that I measured publicity in the *Times* of London rather than in some other newspaper that catered more exclusively to the criminal classes. Reliance on the *Times* is indeed regrettable, but unfortunately unavoidable. As I noted in my paper:

Newspaper publicity devoted to an execution story can be measured systematically and exhaustively only for those newspapers whose contents are exhaustively indexed. The *London Times* is the only London newspaper indexed for the period [under study]. . . . Because the *Times* had a smaller circulation than some other newspapers in London, it is not ideal for the purposes of this study. Nonetheless, it did devote much space to the exe-

cutions under examination, and the number of column inches accorded by the *Times* to an execution story . . . will be used as a rough indicator of the total amount of newspaper publicity devoted to each story. [P. 143]

Zeisel also regrets that I have not compared the effect of executions with the effect of life sentences. The researcher cannot always include all possible studies in a single paper. The study advocated by Dr. Zeisel is indeed very interesting and important. I am currently examining this topic, and I urge other investigators to do so also.

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Review Symposium

The Last Half-Century: Societal Change and Politics in America. By Morris Janowitz. Chicago: University of Chicago Press, 1978. Pp. xiii+583. \$25.00 (cloth); \$7.95 (paper).

THE GIFT OF SOCIOLOGY

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The Last Half-Century is one of those large books that seek to make the vast sweep of contemporary civilization intelligible. Fusing social theory with social criticism, Morris Janowitz follows in an honorable tradition that dates back to the classics of sociological theory. Only the risks of the venture can match in size the grandeur of the book's ambitions. The record shows that few succeed in pulling off that synoptic caper. The attempt to say something profound about such a momentous topic moves sound citizens to apocalyptic brooding. Or it prompts them to latch onto one brilliant gem of a theme, in the hope that it will prove to be the key to unlock the mysteries of the ages. Or it inspires the observer to complicate the most simple things, under the guise of theoretical agility. Janowitz is one of the stout-hearted. He has both the erudition and the chutzpah for this kind of endeavor.

Janowitz aims to account for changes dizzying in scope. After a lengthy excursus on what he calls "systemic theory," he describes three trends which have shaped America between 1920 and 1976: politically, the appearance of unstable regimes; socially, the rise of the welfare state and changes in occupations; and militarily, the era of total war. The list of meaty topics is only a taste of things to come. The next part of the book takes up the subject of social organization, with thick chapters on the hierarchical world of large organizations, the geographical world of local community, the socializing force of legal sanctions, and the pedagogic influence of the mass media. Still, Janowitz is not through. Society transcends the spontaneous process of institutional give and take; with increasing frequency, modern societies attempt to guide their own development. So Janowitz considers the impact on American society of conscious efforts of social and personal improvement, including the psychological sciences, efforts to build strong community organizations, and, of course, the initiatives of the national government.

Like others who have plumbed the meaning of modern society, Janowitz sees a thread of continuity in all the diverse topics. The concept of social

control provides the unifying focus of his analysis. Social control should not be equated with coercion, nor does it mean pliable citizens caught in the grip of the system's needs. Janowitz goes to some lengths to stress that social control presumes a great human capacity for responsible freedom. But coherence simply cannot arise from the clashes of a multitude of free persons. "The concept of social control has been characterized as that outlook which holds that the individualistic pursuit of economic self-interest can account for neither collective social behavior nor the existence of a social order . . ." (p. 29). Janowitz reads the history of America through the prism of declining social control. Its key manifestation is the emergence of "weak or stalemated regimes" which "are unable to produce effective and authoritative policies to manage economic and social tensions and political conflict" (p. 9). Much of the argument of the book turns on the premise that elections are the signal of a society's ability to regulate itself through a democratic consensus. The central imagery of *The Last Half-Century* involves the fight between the forces of control and the forces of anarchic impulse—whether of libido, of individual greed, or of group advantage. Social science figures in the struggle as an analogue to psychoanalysis, playing for society the role that a somber Freudian therapist might play for a patient. Janowitz wants to strengthen the domain of rational control over chaos.

The distinctive strain in *The Last Half-Century* comes from this search for a single principle of explanation and Janowitz's awareness of the variousness of life in advanced industrial America. The tension accounts for the two moods of the book. The upbeat sections never lose sight of the exceptions to breakdown. They owe much to the spirit of Chicago sociology, with its attention to nuance and its ethnographic eye for detail. At this first glance, Americans seem a less alienated, gluttonous, and cartoonish people than the ones who populate many contemporary rightist and leftist jeremiads. Janowitz goes beyond the most glib critics of therapeutic culture who rely on speculation more than systematic data. Despite the faddishness and reliance on a false mystique of expertise involved in the culture of therapy, Janowitz also sees a striving toward self-mastery, an honesty which initiates self-improvement, and an authentic healing impulse. The helping professions represent a "form of benign accommodation of a society in which the pressure for solutions to human problems remains powerful" (p. 441). Various accomplishments in the area of social control match these achievements of personal control. Janowitz notes that the past 50 years have witnessed a decline in infantile political syndromes like anti-Semitism, crusading nationalism, and hard-shell racism. "A more comprehensive explanation can be linked to the process of social control. Since the early 1940s, social control in the United States has had a positive effect on reducing ethnic intolerance" (p. 120). The growth of mutual respect, essential for checking authoritarian movements, makes rational control of development possible.

As is only fitting for a book steeped in Chicago sociology, perhaps the

finest insights surface in the discussion of urban life. Janowitz draws on his own work on the "community of limited liability," as well as the achievements of Chicago-trained urbanists like Gerald Suttles and Albert Hunter. Acknowledging the claim of many that the smooth meshing of the worlds of work and community has declined in the last half century, Janowitz refutes the stark nostalgic contrasts of modernization theory. He holds no truck with those who portray an evolution from cozy traditionalism toward lonely city-dwellers who hide behind their masks of anonymity. "Our systemic model," Janowitz declares, "explicitly rejects the model of socialization implicit in the classical distinction between *gemeinschaft* and *gesellschaft*" (p. 222).

One of the best things about early Chicago sociology was its social realism, the street-wise instinct to shy from grand pronouncements in favor of a firsthand close look at intimate social structures. Its urban ethnography lacked the mathematical precision of a blockmodel; sometimes, it served as a sublimated way to consecrate the humanistic study of man. But the method bore fruit because it unveiled the intricate choreography of contacts linking allegedly estranged individuals in the chain of human association. Janowitz understands that primary relationships are not so evanescent that they disappear when the separation of bureaucracy and community alters the ecology of social life. They are transformed. Or old forms find new tasks when the old ones are parceled out to remote institutions. Janowitz settles for more modest judgments than a declaration that family, religion, and community have dissolved. Residential communities "vary in their stability, diffuseness of boundaries, and internal cohesion" (p. 264). The approach has three virtues. It emphasizes Simmel's insight that the weakening of close-knit networks makes for autonomy, freedom, and creativity. Second, (and here Janowitz borrows heavily from Albert Hunter's work as presented in *Symbolic Communities* [1974]), length of residence, daily social patterns, and the status of parent account for integration in local communities more than master variables like race and class. Third, urban dwellers emerge as feisty beings who show a good deal of resilience in adjusting to shifting environmental demands.

One might think the America of the optimistic pages of *The Last Half-Century* offers some ground for hope. But there is a down side in Janowitz's view of contemporary America. The appreciation for complexity, which serves him well in his analysis of particulars, fades from the appraisal of American society as a whole. The delicate balance between the untidy pluralism of America and the author's glimpse of creeping breakdown is resolved in favor of the perception of anarchy. The caution, the moments of optimism, the qualifications appear in retrospect as a counterpoint to the mainly melancholy view of Americans' ability to regulate themselves and their society.

From the vantage point of the master trend of inadequate social control, America looms as a place of sexual, political, and economic indiscipline. Gluttony serves as a metaphor for the breakdown of restraint in spheres

other than eating. Americans' love of binge-eating, as well as an obsessive concern with bodily weight and appearance, exemplifies a permissive society whose citizens find it painful to discipline their wayward urges. "A strong manifestation of compulsive behavior which reflects an unsatisfying response and which, in fact, borders on and spills over into self-destructive behavior" (p. 410) characterizes the American stress on the joys of eating and wanton devotion to sexual pleasure. Mass communications intensify people's inability to withstand the flood of impulses. Janowitz underscores the appeals to regressive impulses that dominate advertising and argues that television portrayals of mayhem encourage violence. He entertains little doubt about who will be the victor in the fight between ego and id. "In the absence of effective personal and social control, increased material consumption creates only demands for more indulgence. Consumerism implies that the pursuit of material goods in the absence of appropriate moral standards is a destructive act . . ." (p. 348). Janowitz sees a parallel between leisure time conduct and the behavior of economic interest groups. The adversary character of labor-management relations prevents agreement on standards of distributive justice needed to guide a complex society. Group self-interest triumphs over rational planning.

None of these developments alarms Janowitz so much as the disarray of American politics. The most unhappy prognosis for social control comes from the politicians' failure of nerve. In Janowitz's institutional analysis, leaders play a unique role in defining a vision of collective justice and rationality. It cannot emerge automatically from all the shifting about of individual maximizers looking for a score. Only leaders can formulate solutions for the strains of the system. Yet as Janowitz sees it, they have succumbed to a kind of gluttony no less than the electorate. The new breed of politicians forswears the hearty task of leading; they will not brave the dicey search for rational policies. Like binge-eaters, they crave the short-term pleasure of reelection. Hand in glove with public relations specialists, they turn the art of politics into show business. "Sample surveys contribute to an increased emphasis on the imagery and rhetorical style of the political candidate more than substantive content" (p. 531). As a result, "public opinion polls focus the interest of the candidate on the problems and issues of getting elected, rather than creating the conditions for effective exercise of political power after assuming office" (p. 531). Politicians disclose a failure of personal control that parallels the failure of social control evident in the inability of the party system to create a sharp realignment.

Despite his awareness of some partial gains in rationality, then, Janowitz agrees ultimately with countless other analysts of American society that America lacks coherence and legitimacy. But in the process of arriving at that pessimistic judgment, he subtly modifies his original usage of the concept of social control. Formally a term denoting consensual rationality, social control acquires a connotation of virtuous stability. Janowitz's treatment of politics in the age of electronic journalism highlights the conse-

quences of this fateful shift in emphasis, and the conserving disposition that nurtures it.

Janowitz assigns awesome political duties to journalists. Social control demands that citizens give moral approval to authorities, not sullen acquiescence. Yet intimate socialization cannot teach the norms adequate to a complex society. As a result, the media "must" take up the pedagogic slack. Janowitz argues, however, that mass communications have not strengthened "those ego controls which contribute to realistic self-interest and stable group attachments required for effective political participation." They have heightened "projective (and accordingly suspicious) personality predispositions and defensive group affiliations" (p. 347). Television news commentators depict politics as a melodrama of personal feuding between celebrities; they deemphasize "concern with and debate about underlying issues" (p. 359). Equally ominously, the fashions of advocacy journalism simply confirm the unhealthy, free-floating suspiciousness of the electorate and gratuitously add to the mistrust of authority, which makes governing difficult. The net effect, claims Janowitz, is to detract from the public's development of "realism and insight (appeals to ego)" (p. 355). The edifying process of democratic argument succumbs to distraction and manipulation.

One does not have to belittle the problems of unstable majorities or the dangers of converting politics into spectacle to question the terms of Janowitz's formulation. First, the evidence presented elsewhere in the book highlights the diverse and complex effects of the media on voters. Second, by condemning advocacy journalism for generating mistrust of authority, Janowitz elides two distinct phenomena: the media which display failures like Watergate and Vietnam, and the policies and leaders that created them. Janowitz, in effect, blames the messenger who delivers the bad news. In search of a long-term trend, he hurries past specific historical lapses of justice and performance committed by leaders during the 1960s and 1970s. The desire for order at that point begins to stretch the original meaning of social control. Social control, Janowitz claims, is more than a coercive or manipulated stability; it entails personal mastery and a reduction of self-deception. It could be argued, then, that the crisis of confidence of the late 1960s, as well as the tendency of voters to exit from the two major parties, reflected in part an assumption of adult responsibilities, a clear-eyed view of the fallibility of rulers, and the development of personal autonomy. Such a position, however, would involve the recognition that attempts at social control often create instability. Janowitz seems to come down in favor of consent for its own sake, regardless of whether the authorities have earned it. The task of the media, he says, "is to develop a more adequate relation to authority—that is, greater capacity to store and manage aggression" (p. 353).

Janowitz's analysis of the problem of forming coalitions also reveals the functionalist current beneath the surface of his declarations against an oversocialized conception of man. He traces the genesis of stalemate to the

1950s: "The elements of the 'weak' political system had their origins not in the particular political events of 1968-1972 but had been foreshadowed in the election of 1952. . . . For the first time since 1920, the United States government was not based on a relatively unified political regime" (p. 95). But survey data show the most rapid decline in political confidence came during the 1960s. The discrepancy is more than a quibble over a few years. The later date locates the crisis of confidence in public reaction to the bungling of specific rulers and policies. The earlier date shifts responsibility to some gauzy, abstract trend outside of history in "the last half century." The mechanistic premise—different political parties ruling over congress and the presidency equals stalemate—again shows Janowitz's preference for harmony. He never spells out just why the deviating elections of 1952 and 1956 necessarily represented a threat to the system, beyond a lack of partisan symmetry in government. And he fails to examine the informal coalition of Southern Democrats and conservative Republicans which formed a *de facto* majority of sorts.

That neglect matters because it forms part of a larger pattern of ignoring the actual decisions and outcomes of leaders, policies, and coalitions, the better to focus on formal indicators like an election. The emphasis on form rather than content has important consequences. The 1920s, for example, emerge as an idealized time of forceful, hefty majorities. After all, Janowitz's most tangible benchmark of social control is decisive elections. But that contradicts other requirements he lays down as essential for social control. The problem of using elections as the paramount indicator is that it relies on one important, but hardly unambiguous or exclusive, signal of a society's ability to regulate itself in a just and rational manner. If social control truly means the ability of a people to control their own social development democratically, the 1920s do not qualify. Despite strong majorities, the decade was hostile to the statist values implicit in rational mastery of the environment; it was a time when business sought to enhance its own group position with little thought given to the needs of the whole. The point is not to flail capitalists. Rather, electoral stability may coexist with an *absence* of collective mastery. The recent period, which Janowitz sees as one of momentous instability, was also characterized by halting attempts to attain greater social control over corporate investment decisions, national security policy, and the violation of minorities' civil rights. The extension of social control created instability. To recognize this would require Janowitz to give greater attention to impediments to control deeply rooted in American society, like the staying power of the ideology of liberal individualism, forces of political economy, and a unique party system. Such an endeavor might cast some doubt on Janowitz's assumption that "the United States is representative of those 'Western' advanced industrial nations which have been faced with marked strain in their political institutions" (p. 3).

The prospect of melding together in a convincing whole all the loose couplings that form modern society might give pause to a sociologist of

lesser stature or learning than Morris Janowitz. While the obstacles to understanding are clear, a few of them in *The Last Half-Century* have been thrown up by the author. The prose sometimes becomes heavy and flat. In a few places, the breadth of Janowitz's scholarship, a tendency to digress, and the habit of organizing chapters as a procession of theses, qualifications, and retreats blur the clarity of the argument. Above all, the book tries to accomplish too much. Janowitz wishes to commemorate Chicago sociology. He applauds the collective accomplishments of social science. And he delves into the crisis of authority in contemporary Western societies. It is a tall order.

What accounts for the tension, in *The Last Half-Century*, between the simple verdict of breakdown and Janowitz's awareness of complexity? The answer may lie in the broader purposes of the book. Despite the erudite footnotes, the wealth of knowledge, and the difficult language, the work is a personal testimony, a lyrical work whose method of presentation cannot entirely suppress its passion. Throughout the moments of lamentation, Morris Janowitz is witnessing to transcendent values of reason, commitment, and freedom as the ultimate ends of human existence against all that would destroy them. He measures America against this relentless standard, which heightens the perception of the country's disarray. Writing about soap operas, he argues that "the humanistic 'coda' in the dramatization of interpersonal relations is a startling admixture of humor and tragedy—indicating the extent to which popular culture contributes to the trivialization of the prophetic aspect of human existence" (p. 340). Perhaps he strains a bit to find prophetic meaning in trivial if enjoyable diversions, but the point affirms the possibilities of humankind. A conserving impulse runs through *The Last Half-Century*; the values it seeks to uphold are liberal.

The conserving impulse is further evident in Janowitz's view of social science. The heritage of classical sociology helps to fortify the heritage of enlightenment. The former does not simply provide a means to the latter; sociology is the embodiment of enlightenment. True, market researchers who use opinion polls to sell candidates as if they were breakfast cereal debase the trade. But Janowitz applauds the virtues of a value-free sociology and its cumulative wisdom. "I do not believe that, in general, the results of social sciences are devices by which the elite maintain themselves in power. Nor do I believe that the development of social science has contributed to a decline or eclipse of reason and common sense." The last sentence, really a barb thrown at the Frankfurt school, points to Janowitz's broader mission of rescue: to keep sociology from all kinds of fashionable intellectual cults which "serve to fragment academic thought" (p. xi). The synthesizing aim of the book reflects "the task of maintaining a national intellectual culture for sociology and the social sciences" (p. xi) in the face of hostile social and academic forces.

The Last Half-Century shows reverence for more than social science in the abstract; it is also a loving testimony to the community of science

represented by the Chicago school of sociology. It can hardly escape the reader that the book is essentially a compendium of the research of Janowitz, his colleagues, and above all his students. The statement of the preface should be interpreted ritually, in the more sober meaning of the term. "I have written this book for my students—past, present, and future" (p. xi). The book is a gift. Seen in this light Janowitz's assembling of the work of his students and colleagues may be viewed not simply as a narcissistic mirror to his own achievement but as a generous affirmation of the timeless values of human association and the collective search for truth. *The Last Half-Century*, then, also chronicles Morris Janowitz's successful attempt to find reason and meaning in the last half century of American, but especially Chicago, sociology.

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CONTEMPLATIVE THEORY AND SOCIAL POLICY

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Morris Janowitz's tour de force now before us fully deserves the brief but eloquent encomia by Edward Shils and Bernard Barber displayed on the back flap of the jacket. *The Last Half-Century* is a major work of social analysis, in which Janowitz displays the sociologist's technical skills in probing and explaining America's current state while displaying an engaged citizen's concerns for both values and organizational mechanisms. The principal title, though not substantially misleading with respect to the temporal period through which the author traces significant behavioral and structural trends, does require the subtitle to delimit in both space and focus the still-large area of his concerns. That Janowitz has a special interest in American political structure and behavior, and especially in the position of the military in America and in other Western societies, can surprise no one even sketchily familiar with his extensive prior publications on these themes. That he is also concerned here with the changing character of social inequality, pervasive bureaucratization, the mass media, and the trends and fads in psychotherapy demonstrates the breadth and depth of a master scholar doing serious work.

The author's integrative theme is that of social control. He devotes his second chapter to an extensive elucidation of this old-fashioned term. Janowitz, however, wishes to extend the term beyond conformity, which derives from conscientious compliance and/or socially sanctioned reinforcements of varying degrees of severity. He wishes also to include what might be called social problem solving, that is, the ways a community or other social aggregate contains its conflicts and pursues common goals. Here, however, for reasons apparently related to his explicit value preferences for democratic decision making, he seeks to draw a conceptual distinction between social control and coercive conformity. One can share his values without agreeing with the utility of this conceptual tactic, which in effect gets in his way when he later discusses law (and particularly the judicial system) as "legitimate coercion."

A second theme, to which the third chapter of the book is devoted, is what Janowitz wishes to call "systemic analysis." The discussion of this (pp. 53-81) seems to me uniformly ponderous and at times pretentious. The author wishes to trace connections among social phenomena and thus deal with systems. So far so good. He also wants to avoid the restrictive (and empirically untenable) assumptions of equilibrium models represented in extreme functionalism and wants explicitly to deal with sequential relations. That too seems admirable to this reformed functionalist whose own conversion dates back at least two decades. He also wants to avoid containment by overly precious disciplinary boundaries. All intellectual descendants of Auguste Comte must heartily concur. I simply remain unconvinced that "systemic analysis" has the distinctive novelty that Janowitz seems to imply. He later (p. 399) refers to his as a "contemplative perspective." The claim is just and commendable, unless it is intended to be unique and proprietary.

Nominally lesser but perhaps equally significant themes recur. Two of those are related (as demonstrated by "systemic analysis"). Janowitz refers to structural "disarticulation," particularly as exemplified by the spatial and social separation of work and residence (community). Disarticulation, as a broader phenomenon, is a consequence of increased structural differentiation. These combined processes, in turn, have further important consequences: the multiplication of the forms and contexts of social inequality and thus the obfuscation or destruction of distinct and significantly general social strata; all of this, in parliamentary democracies, leading to weak political regimes lacking clear majorities or stable coalitions for resolving public issues and implementing social goals. I believe that this fairly, if elliptically, represents one of Janowitz's central arguments, and it alone would have been worth the price of admission.

Invoking a point of personal privilege, I wish to revert to the discussion of social inequality. Chapter 5, "Social Stratification: Occupation and Welfare," provides an exemplary model of how to deal with the complex and multidimensional character of social inequality in an "advanced industrial society," a type of society of which the United States—partly by reason

of having been spared a feudal past—is an outstanding if not extreme example. I wish that sociologists, particularly those not mindlessly committed to shoddy Marxist cant, could be required to read, and be examined on, this discussion. Janowitz demonstrates the scant and diminishing utility of “class” as a unit of analysis or independent variable for explaining anything. I have one semantic regret. Having explicitly rejected the “stratigraphic” model or metaphor, why did the author persist in using the unfortunate term “stratification” when he explicitly means inequality in its many forms? (Of course, *mea culpa*, but my public penance was long ago recorded.)

Continuing with semantic complaints, I found a dismaying number of errors, few if any of them egregious. I shall provide here some brief examples from a considerably longer array (with my corrections or queries in brackets), chiefly to make a possibly useful point. “There is also [an] arbitrary element in selecting . . .” (p. 60). “The analogy between society (or the social order) [and what?] has been too tempting to avoid” (p. 63). “They are more indirect as [this word should be deleted] in reflecting actual patterns of managerial authority and business norms” (p. 228). “The magnitude of these merger movements . . . has been modest [compared] with that of 1887–1904” (p. 229). “The thrust of these conglomerates was mergers lead [substitute: led] by financial leaders rather than by industrial managers” (p. 230). “No threshold was past [substitute: passed]” (p. 369). I discovered no misspelled words, but far too many instances of words omitted or the wrong, correctly spelled, word used. The point is that proofreading cannot be entrusted to those hired by printers. Only the author *and* the publisher’s editor can be expected—and it is their responsibility—to read manuscripts and proofs for sense. Two further errors, these in tables, illustrate the same point. Table 6.2 (p. 187) shows a figure for “Total Uniformed Personnel” (in the U.S. military) in 1945 of 12,123,455 but an incredible 22,935,107 in 1955. Surely, a 20 million error? Table 6.4 (p. 195) records, in the stub, “Personnel mobilized (per 1,000),” which would mean a ratio, but the numbers recorded are simply rounded to thousands, which is not the same thing at all. Imperfection on the part of the author and editor is a shortcoming with which I as a much-published author can warmly empathize; perhaps also those many critical readers thanked in the preface did not read very closely.

Still a further theme informs the author’s analysis, and that is the mixed, intrinsically limited, but still positively influential contribution of social science to social amelioration and social policy. The last four chapters of the book, constituting part 4—“Rationality, Institution Building, and Social Control”—discuss this set of issues most extensively, but at suitable junctures in earlier discussions the subject receives attention. Janowitz notes circumstances in which social scientific theory and research may add to public confusion, is keenly aware of the limits of rational instrumentalism (he uses the unfortunate term “manipulation” and then must explain each time that he means the concept in a nonpejorative sense), yet remains

convinced of both the utility and propriety of social-scientific involvement in social decision making.

We have, then, a book that is widely and deeply informative about social trends, critically analytical of our current state, and generally wise in analysis, in estimating the future, and in suggesting courses of action.

My regret that it is also rarely if ever witty perhaps reflects only my own propensity to frivolity, on the excuse that the work we do should also be fun.

Book Reviews

Alienation and Charisma: A Study of Contemporary American Communes.
By Benjamin Zablocki. New York: Free Press, 1980. Pp. xxiv+455. \$22.95.
\$22.95.

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National Institute of Mental Health

Alienation and Charisma tells us rather less than it promises about alienation, somewhat more about charisma, and a great deal about communes.

Benjamin Zablocki's introduction makes it clear that he intends not only to use the concept of alienation as a means of organizing his empirical results, but also to use his empirical results as a means of elucidating the concept of alienation. Certainly—as he convincingly argues—there is no reason in principle why the study of so relatively rare a phenomenon as communes cannot be strategically useful in giving theoretical insight into so pervasive an aspect of social life as alienation. Yet, in actuality, Zablocki's study of communes does not tell us much about alienation that we did not already know from research on more conventional topics. It seems to me that he begins with a questionable set of empirical assumptions: "We shall see that, whereas powerlessness and normlessness have been the forms of alienation most endemic to Western democratic societies, meaninglessness and value isolation are the forms that most plague commune members" (pp. 9–10). The book examines only the second half of this assertion; it is the first half that I think doubtful. Zablocki presents no evidence, or any theoretical argument, to support his generalization that powerlessness and normlessness, rather than self-estrangement, meaninglessness, or value isolation, have been the forms of alienation "most endemic to" Western democratic societies. I think the generalization is wrong and that Zablocki therefore gets off on the wrong foot. The failure to provide much new insight into alienation stems also, I think, from Zablocki's failure to juxtapose his research on communes against what has previously been learned about alienation from research on other social institutions. The theoretical backdrop to this book, its title notwithstanding, is not alienation but communes.

Late in the book Zablocki redefines alienation in terms of consensus: "An individual will be alienated from a collectivity if and only if he perceives himself to be outside of the prevailing consensus or if objectively that consensus itself has been lost" (p. 258). This is an intriguing definition. It has the virtue, as he claims, that "defined in this admittedly cumbersome way, alienation becomes that most useful kind of sociological

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variable—defined objectively in terms of the larger social structure but measurable in terms of its effect on individual participants. It has no meaning except insofar as it relates the decision making activities of individuals to the decision making activities of a collectivity” (p. 259). Moreover, this definition suits Zablocki’s data-analytic purposes well, for his analysis does point to consensus as being a central problem of communes. But does this definition suit other theoretical purposes as well? I think not. Zablocki argues (citing reviews of the literature by Richard Schacht, Joachim Israel, and Lewis Feuer) that “where consensus is absent, there is always alienation and, where consensus is present, no matter what social problems arise, the situation cannot best be understood as a problem of alienation” (p. 259). I see nothing in the research literature or in the cited reviews of this literature to sustain this conclusion. Moreover, I think the proposed definition unnecessarily restricts the theoretical sweep of the concept, alienation. Can powerlessness really be reduced to a problem of consensus? Can self-estrangement? More generally, what does such a definition do to the Marxian approach? Even to raise this question is to indicate that a definition fashioned to fit research on small face-to-face groups is ill suited to research on a large-scale industrial society. Zablocki says that his “only purpose [in proposing this definition] is to identify the variable aspects of alienation for sociological research, not to preclude other philosophic or literary uses of the term” (p. 259). He might better have said, “for my own sociological research”; his definition is much too narrow for most other sociological research. Moreover, it would be impossible to operationalize his definition for studies of larger institutions or whole societies. My counterproposal would be: Do not narrow the term, alienation; instead, deal directly with what Zablocki, on the first page of this book, defines to be the central theoretical issue in the study of communes—consensus. But deal with the problem of consensus on its own terms, not by transforming it into something it is not.

If one reads this book not as an inquiry into the social roots of alienation but as an inquiry into the nature of communes, then it is very impressive indeed. It is remarkably successful in studying an unusually hard-to-research phenomenon. It succeeds especially well at a task I would have thought impossible—selecting and studying a meaningful sample of urban and rural communes. No such sample, of course, could be precisely representative of the entire population of communes, nor does Zablocki claim that his is. He has nevertheless developed ingenious methods for selecting a sample of 120 communes that can be regarded as accurately representing the breadth and diversity of communes in this country. This not only is a methodological feat, it also has important substantive implications, for some of Zablocki’s principal conclusions concern the ideological and structural diversity of communes. So diverse do they prove to be that the reader of this book begins to doubt that communes have anything more in common than that they fit his definition: “any group of five or more adult individuals (plus children if any), the majority of whose dyads are

not cemented by blood or marriage, who have decided to live together, without compulsion, for an indefinite period of time, primarily for the sake of an ideological goal, focused upon the achievement of community, for which a collective household is deemed essential" (p. 7). On the face of it, this definition would seem to define a rather homogeneous population, certainly in comparison with all the rest of us who do not live in such households, or whose living patterns are not based on such ideological commitments, or who otherwise do not meet the precise requirements of this properly restrictive definition. But Zablocki demonstrates, to the contrary, that the diversity of structural arrangements, leadership-followership patterns (here is one place where charisma enters prominently into the picture), and objects of ideological commitment is far greater than I, at any rate, ever imagined. He does this historically, statistically, and in rich descriptive detail. No one who reads this book can ever again fail to recognize the immense range of patterns of life that can be displayed by groups of five or more mainly unrelated individuals living under one roof. At its most elementary level, this book teaches us to be wary of any conclusions about communes that do not specify with considerable precision what types of commune provide the evidence and to what patterns of human activity the conclusions can properly be generalized.

At a second level, Zablocki provides a detailed analysis of the personal characteristics and ideological beliefs of communitarians. These psychological and ideological portraits are somewhat less than fully convincing, both because the comparison groups are not well chosen and because Zablocki evidences a naïveté in using quantitative methods of analysis of survey data that is astonishing when contrasted to the sophistication he shows in methods of sampling and in sociometric analysis. This naïveté is most striking in his persistent failure, one might almost say refusal, to develop composite indices, for example: "The self-esteem of the stayers decreased or stayed the same on seven of the eight items of the Rosenberg self-esteem scale. The self-esteem of the leavers, on the other hand, increased on seven of the eight items" (p. 140). Why would anyone dismember a well-validated scale into its component parts and summarize the data in this cumbersome and uninformative way? Still, if one examines the detailed information presented in the tables, it is apparent that his characterizations of the communitarians cannot be too far off the mark. Certainly the data amply bear out his principal conclusion of ideological heterogeneity; more than that, they support his conclusion that the problem of commitment is central to the lives of these people.

At a third level, Zablocki presents an intriguing analysis of the role of charismatic authority in the unstable equilibrium of commitment and social control. To do this, he redefines charisma "not [as] a locus of supernatural traits in a particular individual, but [as] a transactional process occurring with predictable regularity in collectivities searching for consensus" (p. 271). He then goes on to say that "the primary function of charisma is to provide structured opportunities for the investment of self. The

primary function of the charismatic leader is to provide values that evoke in each follower the desire to invest self and, having invested, to refrain from withdrawing self again" (p. 274). The evidence that Zablocki brings to bear to demonstrate that such processes do occur in communal settings is only suggestive. But the insight this analysis affords into processes of ideological commitment and social control in charismatic movements may have applicability far beyond communes.

Perhaps the most ingenious analyses of the book concern communal instability, certainly an important issue in the life of communes, which are notoriously unstable both in terms of "a high probability of disintegration" and in terms of "a high membership turnover." As Zablocki puts it, "Communes are typically but not intrinsically unstable" (p. 146). Given the nature of this study—an initial assessment and a one-year follow-up—what he has to say about the disintegration of communes is a bit too inferential to be entirely trustworthy; his principal analyses are based on synthetic cohorts, which can provide only inconclusive evidence. But he can say a great deal about the high rates of population turnover that he finds in many communes. The single most powerful predictor of the rate of population turnover in communes is "dyadic cathexis, a measure of the density of reciprocated loving relationships within a commune network. The direction of the relationship is surprising; the more love, the greater the turnover" (p. 185). Surprising though the finding is, Zablocki makes sense of it. As he says in another context, "Something about a commune does not love a dyad" (p. 121).

Which brings us back to the central issue: consensus. Zablocki concludes by saying, "The most general overall finding of this book is that communal systems, in their search for consensus, tend to generate intense networks of interpersonal interaction, which, in the absence of periodic charismatic renewal, tend to be associated with communal instability" (p. 354). This conclusion is pieced together out of some evidence that I think well documented (in particular, the analysis of networks of dyadic relationships) and some that I think only inferential, albeit plausible (in particular, the synthetic cohort analysis of the disintegration of communes). One cannot take the conclusion as by any means proved. Yet, it would be hard to read this entire book—statistical analyses, descriptive materials, and historical background—and not find this conclusion consistent with the evidence and very likely true. If true, it is also important, not only for what it tells us about communes but also for what it tells us about other institutions, particularly those that involve ongoing face-to-face relationships. Zablocki points to similarities between his findings for communes and those of Amitai Etzioni and Mark Granovetter for formal organizations in suggesting that "too much bondedness can be as harmful for an organization, even for a commune, as too little bondedness" (p. 356). Perhaps his insights into the role of charismatic authority in the search for consensus likewise may teach us something important about the rest of society.

The Dominant Ideology Thesis. By Nicholas Abercrombie, Stephen Hill, and Bryan S. Turner. London: George Allen & Unwin, 1980. Pp. x+212. \$28.50.

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The main issue in *The Dominant Ideology Thesis* is a perennial of the left: the failure of the proletariat of advanced capitalism to sustain any interest in revolutionary socialism. Marxists have considered many reasons for this historical snub. Prominent among them has been the contention that the massive ideological apparatus of advanced capitalism has so thoroughly beguiled the working class that it has become a willing and even enthusiastic accomplice in its own exploitation; capitalism has become a victim-pre-empted crime. Nicholas Abercrombie, Stephen Hill, and Bryan S. Turner are critical of this analysis, which leads those who accept it either to exasperation and then contempt or to a belief in a greatly enhanced role for the intellectual vanguard. Instead, the authors claim that Marx's reference to "the dull compulsion of economic relations" remains the best explanation for the quiescence of the working class. They also identify an odd convergence between the (largely Frankfurt) Marxist notion of ideological hegemony and the Parsonian emphasis on value consensus. This functionalist approach is contrasted by the authors with Durkheim's emphasis on functional interdependence in *The Division of Labor* and with Weber's recognition of the constraints of capitalist economic structures (despite Weber's emphasis on the Protestant Ethic as a source of value consensus). They thus take issue with what functionalism has made of the sociological tradition as well as with main currents in neo-Marxism.

In arguing that economic compulsion, not ideological conversion, accounts for the relative passivity of the working class, Abercrombie, Hill, and Turner provide an interesting historical overview of ideological hegemony, or rather of its absence. Drawing from an impressive array of contemporary sources, they argue that Christianity penetrated only marginally the illiterate and geographically isolated peasantry of feudalism. Christianity, as well as the doctrines of chivalry and courtly love, functioned mainly to integrate and control the dominant class. The authors make much of the role of the dominant ideology in securing the family as a property-holding institution. But the peasant masses lived their own lives: they spoke the vernacular, were largely pagan, and were prone to frequent hostile outbursts that were contained only by the military superiority of the aristocracy. Similarly, it is argued that the ideology of the bourgeoisie did not penetrate the working class of early capitalism. Chartism, Owenism, and the vigorous trade union movement are adduced to show the resistance of the working class. Whereas the capitalist class was drawn to Malthusianism, the dismal science of economics, and utilitarianism, the working class

was reformist and egalitarian. Moreover, even when the working class seemed to be co-opted by the dominant ideology, the authors claim that their beliefs displayed a distinctive working-class twist: the Victorian notion of self-improvement had a corporate and collectivist emphasis in the working class. They further claim that the Halévy thesis that Methodism pacified the working class is belied by the fact that most of the working class participated in no organized religion. Again, they claim that the dominant ideology served to integrate and control the dominant class and that illiteracy and geographic and social isolation prevented any substantial indoctrination of workers. A distinction is drawn between the working class's factual and realistic acceptance of its circumstances and the normative acceptance of a fully indoctrinated subordinate class.

The real crux of the issue of ideological incorporation is, of course, the character of the working class under late capitalism. It is in the 20th century, according to the thesis of ideological incorporation, that the proletariat has succumbed to the din of the mass media and the baubles of the consumer society. But the authors point out that the absence of a radical class consciousness does not mean that the working class has become an enthusiastic supporter of the status quo. For while the modern worker remains for the most part politically conventional, it is argued that, rather than arriving at value consensus of the Parsonian or Frankfurt variety, the contemporary working class maintains a dual consciousness—nothing like a revolutionary consciousness but something far short of an enthusiastic acceptance of the (internally fractured) ideology of capitalism. Noting that the modern state has significantly limited the traditional prerogatives of property ownership, the authors cite a number of attitude surveys that show, for example, that a slight majority of workers are skeptical of the fidelity of capitalist democracies to their own liberal principles, that they believe that big business is too powerful, that the law treats the poor differently from the rich, and so forth. They also cite a study of working-class tenants in London that indicates their approval of squatting and their belief that the rights of property owners should be abrogated in the face of human needs. Several ethnographic studies that find an antimanAGERIAL spirit in the shop are also cited. The book claims, then, that structural constraints along with a steady stream of genuine rewards, rather than false consciousness, explain the relative acquiescence of the contemporary working class: "Our position is that the non-normative aspect of system integration provides a basis of a society's coherence, irrespective of whether or not there are common values. Social integration and system integration can vary independently. Social classes do have different and conflicting ideologies but are, nevertheless, bound together by the network of objective social relations" (p. 168). In other words, economic and various dimensions of social power, not ideological manipulation, explain the hegemony of the dominant class.

The most serious shortcoming of the book is one of method. How does

one know if the working class accepts the status quo? Or in what sense? The attitude surveys cited are feeble instruments for such a task, as they are unable to distinguish between rhetoric and effective social beliefs, that is, the beliefs that are operationalized into a political praxis. Just what does it mean if workers grouse about their bosses and landlords and do little else? The authors show some awareness of this problem on page 189 when they consider the possibility that "ideology cannot be treated *purely* as a category of consciousness" and suggest that "the solution is to see ideologies as sets of practices, or as real social relations." But this more sophisticated conception of ideology as a form of social praxis is not incorporated into their analysis, and hence their comments on late capitalism are, paradoxically, less convincing than their material on epochs for which we do not have the dubious advantage of attitude surveys. Apart from the issue of whether Marx's analysis of capitalism is strong enough to warrant an endless stream of interpretations of the failure of the working class to do as predicted, the authors would have done well to engage in the more sophisticated and demanding forms of political and social analysis that characterize Marx's substantive work. Almost nothing is said about the role of unions under late capitalism, and this is a pity, for that kind of institutional analysis would have been far more revealing of the politically effective consciousness of the working class than any compendium of attitude surveys.

Power, Paradigms, and Community Research. Edited by Roland J. Liebert and Allen W. Imershein. Beverly Hills, Calif.: Sage Publications, 1977. Pp. 343. \$22.50 (cloth); \$9.95 (paper).

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Power, Paradigms, and Community Research consists of five papers presented at the International Sociological Association World Congress in 1974, one presented at the ASA meetings the same year, and two which, apparently, were solicited by the editors. Students of community power are likely to find modest increments of knowledge offered by several of the selections as well as considerable overlap with other materials published by some of the contributors in monographs and the major journals. In fairness to the authors of the papers, their works were more innovative at the time of their original presentation—1974—than they are today. The volume bears a 1977 copyright, but the back cover lists four other volumes in the same series which were published later, and the book was received by this reviewer in February 1981.

An introductory chapter by the editors makes a valiant attempt at integrating the diverse materials that follow, by identifying three "emergent paradigms": (1) a macro-level structuralist approach treating communities

themselves as the units of analysis and aggregate characteristics as key variables, (2) a micro-level concern with the social definition and individual experience of powers, and (3) a focus on the dynamics of power as expressed through the exchange of resources in a relationships network or system. The editors note the possibility of a fourth paradigm—the Marxist or critical theory perspective—which is not included in the volume.

Still another area of current concern—the “vertical-axis” notion that local decisions are at least constrained by extralocal economic and political units, if not made by them—is not identified as an emergent paradigm in the volume or given central emphasis by any of the contributors. Nonetheless, four of the papers (by James Lincoln, Herman Turk, Jeanne Becquart-Leclercq, and Peter Marsden and Edward Laumann) collectively offer as much new insight into this notion as most of the other materials add to our knowledge of the paradigms identified by the editors.

That concepts from other research traditions may be applied successfully to describe both local and extralocal decision-making parameters is illustrated by Lincoln's application of the ecological concept of dominance to the structure of the networks of organizational units within and among communities. While Turk's focus is primarily on the effects of interorganizational structure and characteristics on policy outputs within communities, extralocal linkages are discussed, and the essay is pregnant with intercommunity inferences. Intersystemic connections are given the greatest emphasis by Becquart-Leclercq, in a primarily descriptive paper, “French Mayors and Communal Policy Outputs.” Her research shows how the “relational *reseau*” of personal contacts and influences connects the local system with its environment and enables a mayor to obtain more for his commune in terms of outputs. Finally, Marsden and Laumann, in an exchange analysis of issue resolution, identify 10 resource bases of influence including outside (extralocal) political and economic ties. Examining the relative control over various issues vested in persons controlling various levels of resources in a U.S. community, they find that ties to external economic actors are one of the two most consequential resources over all issues. Had they not employed a closed-system model, it is likely that external political ties would have been equally or more important. While Marsden and Laumann's paper focuses on a variant of a purposive action “market” model developed by James Coleman (in a very general and abstract essay also in this volume), it concludes by noting some points of tension between that model and the network approach advanced elsewhere by Laumann and others. Among these is the problem of system permeability: “Nondecisions or sharply restricted ranges of alternatives may be imposed on the community level by constraints created by decisions made at higher levels in a nested set of interdependent systems” (p. 239). Certainly concepts other than this one or the three suggested by Liebert and Imershein could be identified by the interested reader, who would thus conclude that the paradigms selected for emphasis are not inclusive.

At least two of the contributions are only peripherally related to community power and seem somewhat out of place in the context of the editors' introduction and the other papers. Eugene Litwak, Henry J. Meyer, and C. David Hollister examine the linkages between formal bureaucratic social service organizations and community primary groups, such as families and neighborhoods. Their conceptualization of critical contextual elements (nature of the task to be performed and the social distance between the primary group and bureaucracy) is clear and the ideal linkages presented are persuasive (and documented by case studies), but the discussion would better serve audiences other than the one this volume is likely to attract. Less distant from the topics covered by most other authors, but still not comfortably placed in the volume, is William John Hanna's preliminary framework for improving the understanding of the relationships between politics and the quality of urban life. After a brief discussion of political actions as independent variables, the bulk of the article concerns the conceptualization and measurement of quality of urban life.

The volume is rounded out by an extremely comprehensive but technical essay by Ronald Burt. Burt's treatment of control of resources as the bases of power and the network of general influence relations as the manifestation of power was, for me, one of the most stimulating contributions in this anthology. While several of the basic ideas are scattered in several other publications by Burt, they are nicely integrated here and convincingly tested (using data from Laumann and Franz Pappi's study of a German community) in an essay that would be too long for publication as a journal article. Had a few of the essays in this volume been deleted and other authors been given the space necessary to develop their ideas as fully as Burt, the net contribution of the volume would have been greater. As presented, the anthology is uneven and too diffuse in its scope. Nonetheless, readers with keen interests in community power will find some of the materials stimulating and useful.

Community Power Succession: Atlanta's Policy-Makers Revisited. By Floyd Hunter. Chapel Hill: University of North Carolina Press, 1980. Pp. xx+198. \$15.00 (cloth); \$9.00 (paper).

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In 1953 Floyd Hunter published *Community Power Structure: A Study of Decision Makers* (Chapel Hill: University of North Carolina Press) and literally established a new field of study. Most researchers in the field today are willing to call Floyd Hunter a "founder" and his book a "classic." Hunter described Atlanta, Georgia, setting forth the names and the behavior of decision makers in the life of the community. Among his methods was the

so-called reputational method for identifying top and lower-ranking leaders. He described the power structure of the black subcommunity. He identified values and roles of institutions and associations as they related to community power. He followed issues and described the more private aspects of power. His startling findings and his "new methods" brought forth a storm of controversy and a plethora of studies by political scientists and sociologists. Journalists joined the procession. In fact, after Hunter's work, almost anyone might set forth his own power-structure speculations carrying personal assurance that he or she really knew who ran the town.

Now comes *Community Power Succession*. Hunter has revisited Atlanta 20 years later and, following many guide lines set forth in the earlier study, he has collected data and probed for answers about the current structure and the changes to be observed. Let it be said at the outset, Hunter did not intend that this book would be read by only those interested in Atlanta, Georgia, or in community power structure as a field of study. It is true that the succession of power in the community is one theme. But the major emphasis is on the implications of power structures on the traditional tenets of democracy in the national society. Hunter emerges as an impartial scientific observer with unique interviewing abilities. But more than that, as political theorist, he has aligned all the data to explain the phenomena of power and its impact.

Hunter never "pussyfoots" or engages in double talk. He begins at the outset with his personal assertions: "The system of institutional policy-making is gravely imbalanced, skewed in favor of economic institutional values. It is visible at all levels of societal life, community, national, and international" (p. xiii). "The majority of the major decisions that direct our lives are directed from the control centers of the private enterprise sector of America" (p. xiii). "I firmly believe that the malfunctions of policy-making can be corrected by broadening the base of political participation" (p. xiv). "The ordinary citizen must have open access to participation in all political decisions, whether public or private, which vitally affect his well-being" (pp. 123-24).

Hunter has drawn up some 11 hypotheses to guide his research as it bears on societal structures and some 20 hypotheses to embrace power in society. "Study II," as he calls the present volume, covers all of the same subjects as the earlier book. These include: power and its impact on the physical structure of the city; powerful families; continuities of the community power structure in Atlanta; the black community; power succession; political perspectives; private power; projects, issues and policies; patronage; and effective community change.

No one can stamp Hunter with a given method of investigation—except that almost everyone does. "Oh, Floyd Hunter, he always uses the reputational method to determine a community power structure." Yes, he does—and, according to my documented count, over 21 other methods critics seldom discuss (list available to interested readers).

Hunter has more than multiple methods. He has a distinctive style, marked by scientific detachment and by strong opinions. This style is revealed in incisive interviews with key leaders and informants. If there is any one word that can describe this style, it must be "probing." At one point he says that he went to Atlanta with 150 questions for which he sought answers. Hunter wants to know the "meaning" of his data; he wants to know the implications of community power structures for the larger society and especially for the operative democracy. He believes his findings are important. You know that he cares and wants to be a voice.

In both the 1953 and the 1973 studies, higher-ranking business and professional persons emerge as most powerful in the community power structure.

A high "inheritance" of power was found in both studies: five of the 10 key leaders of Atlanta (1973) deemed by their peers as most powerful had succeeded a member of their own family in their present position; seven, or nearly one-fourth of the 26 next lower ranking persons, held power as an inheritance. In sum, (as Study I also showed) a high proportion of the very top power figures inherit their bases of power with a 50:50 ratio of status ascription and achievement among the top 10.

Role changes have occurred in the black power structure (Atlanta has a black mayor and a few rising business owners), but such changes have not perceptibly shifted the industrial and technological hold on power by the larger Atlanta power structure. The policy goals of the black power structure have coincided with those of the white in the general maintenance of the larger American system.

One can agree that the economic dominants have the major influence on vital community decisions, that the rich get richer, that major semimonopolistic price mechanisms govern, and that the traditional concepts of democracy are in danger. What to do about it? Ah, there is the rub.

Hunter does not hesitate to offer his prescription: get behind the cooperative movement as "one of the most possible directions for profound change in a broad spectrum of community relationship" (p. 165).

Hunter admits that the co-op movement is not a panacea. But the central question is whether this movement is economically viable and politically feasible in the highly industrialized, technologically advanced society. Hunter does not describe how producer co-ops sometimes become major antagonists to consumer interests.

So be it: the dilemmas of modern industrialized societies and those of the agrarian mold constitute a challenge to all social scientists. The implications of Hunter's findings may be debatable, but the findings themselves are becoming ever more difficult to refute. The old clichés are all well known: Hunter's reputational method is defective—Atlanta is unique among cities, other cities are entirely different. These clichés are harder to maintain since the publication of *Community Power Succession*. Hunter has lived long enough to observe that the voices of his critics have become stilled.

Authority. By Richard Sennett. New York: Vintage Books, 1981. Pp. 206. \$4.95.

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Richard Sennett describes *Authority* as "the first of four related essays on the emotional bonds of modern society" (p. 3). The other three will treat solitude, fraternity, and ritual.

We all have some intuitive sense of what "an authority" is: Sennett asks us to imagine a Montoux or a Toscanini conducting a symphony orchestra. The qualities possessed by such authorities are "assurance, superior judgment, the ability to impose discipline, the capacity to inspire . . . both fear and awe" (pp. 17-18). Beyond such obvious cases, the difficulties of definition begin, difficulties rooted in understanding the kind of strength on which authority is based. When we leave orchestra conductors and go to parents and bosses and politicians, then we are no longer sure that the strengths which give these figures authority are in the service of any ideal other than domination.

And that is Sennett's theme. He is convinced that "the dilemma of authority in our time . . . is that *we feel attracted to strong figures we do not believe to be legitimate*" (p. 26), figures who will use their hold over people for selfish and destructive ends.

Sennett then analyzes two characteristic modes of modern authority, paternalism and "autonomous" authority. The first claims to care for the persons subject to it, but in fact its professions of nurturance are revealed as hollow whenever the interests of the authority clash with those of the subjects. The second professes no care for the subjects at all but claims only to aid them in advancing their own interests while, of course, still keeping power and exercising control. The first is false care, the second no care. Both bewilder and deprive their victims. Both also make it extremely difficult for the subjects to see the falsity of the authority's claims, thus making liberating responses unlikely.

About half of Sennett's work is devoted to an analysis of these malignant bonds of authority, and here, I think, are found the strength and richness of the book. The author moves with easy assurance between the private and the public, the psychological and the sociological, the historical and the contemporary. The result is as enlightening a treatment of its topics as any known to me. Sennett is particularly good at showing how in our very efforts to reject paternalistic and autonomous authorities of the modern kind we end up tied even more closely, trapped even more tightly, by that which we struggle to negate.

Then, with some help from personal case studies and the writings of Hegel on master and bondsman, Sennett asks how we might begin to make authority less malignant and more nurturing, less concealed and more "legible and visible" (p. 168). He proposes five methods for "disrupting the

chain of command," for doing something to right the imbalance of will between those who command and those who obey. In summary form, the five are: require authorities to identify themselves by name and speak in the active voice; engage authorities in open discourse about the categories they use in making and applying rules; argue for the legitimacy of a variety of obedient responses to directives; practice role exchange; and permit subjects to make open, direct claims for nurturance to their authorities. Sennett proposes these techniques for rattling the cages of large-scale authority because, he argues, they are the methods that work on the small scale, in the personal and intimate settings where one struggles to understand, to expose, and to overcome illegitimate authority.

The book has many virtues. It is lucid and thoughtful. Sennett moves with assurance through modern social theory. No wild calls for total transformation are made, but neither does the author wallow in cynicism and passivity. The scholarship is both broad and solid and always fair-minded.

And yet I must say that the book seems to me off center. Are paternalistic and autonomous authority really the typical, characteristic forms and structures in which malignant authority appears in modern society? What about the legions of technical and scientific experts who authoritatively set so much of the direction and content of modern life with their canon of efficiency and their subservience to profit and power? What about the huge military, police, and spy forces which claim to defend us against our enemies, open and hidden? What about the apparatus of the "helping professions," with their claims to manage larger and larger groups of people as deviants and problems? The idols of the mass entertainment world? And would not a rightly focused discussion of paternalistic authority deal not with George Pullman and his company town but with, say, the politicians and trade union leaders who claim to care and speak for the workers and the poor, or with some of the more strident and self-righteous leaders of the Moral Majority?

Had the book dealt with *these* false authorities it would have come closer to the center of the problems of malignant authority than it has. And at that center, thought must address political, economic, and moral questions the emerge only in blurred shapes and at the periphery of this book.

Varieties of Civil Religion. By Robert N. Bellah and Phillip E. Hammond. New York: Harper & Row, 1980. Pp. xv+208. \$14.95.

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Varieties of Civil Religion consists of five independently written chapters on civil religion by each of the authors, Robert Bellah and Phillip Hammond. The essays focus on America: comparisons of Japan and America, on the one hand, and of Mexico and America, on the other; new religious

movements in America and civil religion; religious pluralism, law, and the differential development of civil religions; and aspirant civil religions of modern Italy. Little overt attempt is made to reconcile or even to explicate the differences in perspective between the two authors, whose writing styles range from the more or less cool and analytic to the hortatory. The result is a potpourri.

Robert Bellah deserves a considerable accolade for having placed—through a mixture of flag-waving and analytic prescience—the civil-religion theme on the modern sociological and societal agenda; while Phillip Hammond has appeared over the years as an assiduous participant in Bellah's venture—mainly by echoing the latter's insistence on the reality of American civil religion and, more innovatively, by striving to specify the institutional vehicles and structural facilitators (as opposed to the symbolic-rhetorical "reality") of civil religion. Much of what they have previously written about civil religion appears with revisions in the present volume.

The two best pieces are Bellah's on Italy and Hammond's on Mexico and America. Bellah—rather ironically in view of his previously stated conviction that every society has *a* civil religion—analyzes the five civil religions of modern Italy. This is a stimulating essay about the relationship among Italian popular religion, Catholicism, Liberalism, Socialism, and Fascism. The failures of the other four to overcome the particularisms celebrated by the first are explored, mainly via the writings of prominent Italian intellectuals. (Bellah does not state effectively why Communism does not qualify.) Bellah's sympathetic invocation of Salvatorelli's characterization of the liberal, Mazzinian perspective is particularly revealing: "Once the new society has really been constituted, there will be no more reason for the separation of Church and state, or of political and religious institutions. . . . No divorce between heaven and earth; our work on this earth is a sacred task, the realization of the reign of God" (p. 99). This strongly implies that the civil-religion theme has to do with what Bellah calls "the central issues in the sociology of human existence" (p. 115). According to Bellah, the culture of societies—certainly American society—should "have a firm commitment to the quest for ultimate reality" (pp. 185–86).

Bellah's concern with the large themes of morality versus corruption, human existence, and the centrality of values in the operation of societies contrasts with Hammond's circumscribed response to the question: Why civil religion in America but not in Mexico? For Hammond—at least in that essay—civil religion is found to be contingent upon the circumstance in which churches compete with each other in religious terms and take a religious, rather than a political, stance toward government (if directly concerned with the latter at all); while government confines its activities to the political realm and, at the same time, holds "the strings" which permit the churches to fly "any religious balloon" (p. 71). Such a circumstance has obtained in the United States but not in Mexico. Thus, in a

special sense, religion and politics in the United States are relatively close, enabling—largely because of politicians' lack of felt need to compete politically with the churches—those in the political realm to employ a religious language that transcends particular religious stances and that defines America spatially and historically.

While agreeing that, in the American case, civil religion has to do with "a distinct set of religious practices . . . that address issues of political legitimacy and political ethics but that are not fused with either church or state" (p. xi), Bellah refuses to limit the applicability of the concept of civil religion to such a circumstance, by reminding us of premodern or regressive "solutions" to "the religio-political problem" (p. vii). Bellah is not even willing, it seems, to see civil religion in modern, Western societies as limited in the manner suggested by Hammond, who states that he wishes to limit severely the applicability of the concept. And yet not only does Hammond produce a typology (p. 44) which admits of civil religions in types of society other than that displayed in his discussions of America, he also argues in the last two contributions to this volume that civil religion has to do with the problem of the individual versus society—in his terms, the problematic and ubiquitous issue of civility.

Hammond says that all societies have a need for civility (linking self-interest and the collective good) because society is like a house, requiring that the rules of civility be followed in the same way that the rules of carpentry must be followed in order that the house will stand. Aside from noting that houses have, *inter alia*, architectural attributes and that they are not made to stand by the continuous efforts of individual carpenters, it has also to be said that if it is civility which makes societies possible and if civility involving what Hammond calls "self-regulation" encourages civil religion, then surely the latter has been and remains a very widespread phenomenon. Hammond's methodological individualism—which is appropriate to the American scene—sits uncomfortably with his invocation of Durkheim and not at all squarely with his own previous declaration that civil religion has to do with the religion-politics problem.

In the aggregate we are told that civil religion has, *inter alia*, to do with morality, ultimate reality, church and state, religion and politics, societal identity, and individual and society; although here and there attempts are made to narrow down the field of topics. Indeed, it may well be that civil religion as the name for a sociological field of problems ought to embrace all of these and even additional themes, particularly in view of the fact that references to what Hammond calls "cultural crisis" have been frequent in the civil-religion debate. There is, however, a great difference between, at one extreme, the labeling of a cognitive interest and, at the other, the attempt to discover the determinants, correlates, and consequences of an empirical phenomenon. Civil religion may, I fear, collapse under the weight that some of its major advocates are imposing on it. This volume contributes much to that weight, even though parts of the work have been carefully and persuasively constructed.

The Celebration of Heroes: Prestige as a Social Control System. By William J. Goode. Berkeley and Los Angeles: University of California Press, 1978. Pp. xv+407. \$18.50.

Raymond W. Mack
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Columnists whose calumnies run to the use of the word sociology as a synecdoche for obfuscating and graceless writing should be fed doses of the prose of William J. Goode. *The Celebration of Heroes* is provocative, informative, and fun to read for such sharply phrased observations as this: "Precisely because all of us spend some of our energy in persuading others we deserve more esteem than we seem to be getting, we also have some stake in refraining from an overly strict examination of others' pretensions, lest our own be pitilessly exposed. Few of us are so virtuous or so obviously superior in our achievements and qualities as to need no charity when we succumb to the temptation of touching up our self-portrait to our own advantage" (p. 237).

Goode has contributed a basic work in social theory by scrutinizing the social order at the intersection of two crucial variables: social organization and stratification. By raising the dual questions of what is socially valued and how meritorious performance is rewarded, he illuminates core issues for sociological scholarship.

The author analyzes systematically the data suggesting how respect is distributed, how prestige is related to ascribed status or to achievement, and how the awarding of prizes and punishments has consequences in the allocation of personnel and power in organization.

For me, the most fascinating chapters are those on the subversion of prestige systems—both the dynamics of subversion and the structural bases of subversion.

Goode's chapter entitled "The Problem of Justice" is an understandably moody one. He concludes that:

Over world history, most people have not enthusiastically pursued justice for themselves for very long; that pursuit is fraught with danger. Power-holders have repeatedly demonstrated their willingness to kill, in their preference for order over justice. Because those who have felt most strongly they were not obtaining justice have also been the weakest in the society, they have not, except in unusual conditions, been willing to risk their lives for it. Those, by contrast, who believe most strongly in the existing social arrangements are also the most likely to be gaining advantages from that system, and therefore they feel the least motivation to alter it; happily for them, they have the greatest power to resist any reduction in their privileges. Preferring order (with its continuing advantages) to justice, they are more likely than others to achieve their preference. As for the less powerful: Whether or not most people in most societies prefer order (as seems likely, since order is necessary for any kind of social life at all), they get it just the same, since it is imposed on them. [P. 375]



The Celebration of Heroes is an intellectual tour de force. Strands of historical data, psychological research, political theory, and philosophy are woven skillfully into a fabric of sociological theory relating institutions and control systems. Many a research project will be spawned from the insights provided in these pages.

In his concluding chapter, Goode has trouble reconciling the democratic value of meritocracy with the notion of prestige as a scarce commodity. Don't we all?

The New Class? Edited by B. Bruce-Biggs. New Brunswick, N.J.: Transaction Books, 1979. Pp. xx+229. \$16.95.

Paul Attewell

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The expansion of universities since the sixties, the growth of professional and technical occupations, and the political strife of the late sixties and early seventies have led several commentators to announce the birth of a new class in contemporary capitalism. Several books have appeared which examine this class from a left perspective, most notably those by Barbara Ehrenreich and John Ehrenreich and by Alvin Gouldner. Now we have *The New Class?* edited by B. Bruce-Biggs, a collection of essays that stem mainly from the "New Right" and deal with the same topic.

B. Bruce-Biggs introduces the collection with a useful intellectual history of the concept of the "new class." Unfortunately the term has been used to designate an extraordinarily broad range of strata, from James Burham's and J. K. Galbraith's ideas on a technical-managerial elite to Irving Kristol's and Norman Podhoretz's focus on academia and government bureaucrats. This fuzziness bedevils the whole book: each author is obligated to define what he or she means by the term "the new class," and this leads to repetition or confusion or both.

The clearest essays are those which are most polemical. Both Norman Podhoretz and Aaron Wildavsky provide essays which critique the new class from neoconservative positions. Podhoretz begins by raising a "perplexing puzzle," the fact that some of the "most privileged elements of American society" expressed "virulent hostility toward their own country" in the sixties. A subsidiary puzzle is that some student attitudes were ratified by "as it sometimes seemed the entire membership of the academic and journalistic professions" (p. 20). This in turn is explained by an adversary culture of U.S. intellectuals who critique capitalism for its philistinism, materialism, and encouragement of selfishness.

A major thrust of this neoconservative critique is to discredit the adversary culture, and indeed liberalism in its political and social forms, by discrediting the motives of its adherents. Podhoretz and his colleagues suggest that liberal idealism was and is a front: it masks a lust for power on the

part of intellectuals who hate bourgeois society because it refuses to be ruled by intellectuals (p. 26).

A similar line of argument explains political liberalism as an expression of economic self-interest on the part of liberals. Aaron Wildavsky develops this idea most thoroughly, although the argument is echoed by several other contributors. The problem of the new class, according to Wildavsky, is that its members lack distance from lower-class competitors in terms of income, prestige, and privilege. Hence the politics of this class (liberalism) should be read as an attempt to raise the new class above the common crowd. Environmentalism is a way of increasing jobs for new-class government regulators. Industrial safety laws result in a decline in gross national product "thus slowing up mass entry into the middle class that creates competition for position. . . . Job substitution means less competition. Besides, as production falls, new-class employment rises" (p. 150). Immigration policy is a way of obtaining cheap maids for the new class, and limitations on campaign contributions give political entrepreneurs from the new class a better chance of being elected.

The reduction of social, political, and religious ideologies to simple reflections of economic self-interest used to be called "vulgar Marxism." However neo-Marxists have learned the Weberian lesson that ideal and material interests combine with ideologies in complex ways, that superstructure is not simply a reflection of the base. The neoconservatives, by contrast, seem to be stuck in a late-19th-century Marxist rut.

Luckily, two chapters by S. M. Lipset and Everett C. Ladd, Jr., contain numerous nuggets of data which demonstrate that reality is more complex than the neoconservative view. While Bruce Biggs writes, "No one seems to disagree that most intellectuals have a highly critical view of society" (p. 92), Lipset shows that about 30% of university faculty members call themselves conservatives, another 20%–37% (depending on the survey) call themselves moderates, and only 5% consider themselves "left" (p. 71). Left-liberals *are* found in high concentrations in the social science faculties at major research universities. They are a highly visible minority, but are they a class?

Ladd shows that the political views of the new class (defined as college-educated professional, technical, and managerial workers) are far from homogeneous. While liberalism in terms of attitudes toward abortion, homosexuality, the death penalty, and so on, clearly is higher among the new class than elsewhere, large proportions of the new class are not liberal, and there are strong age effects. Perhaps most important, once education is controlled for, occupationally based differences in attitude disappear. This is damaging to the new-class theorists, since they focus on the idea that certain occupations constitute the new class. Nevertheless Ladd sees his data as generally supporting the notion of the new class, in that the views of college-educated people are on average well to the liberal/left side of those without college educations.

The best critique of the concept of a new class is to be found in Daniel

Bell's essay. Bell makes several points worth repeating. First, intellectuals are found in various institutions from government to universities to private enterprises. There are relatively few common interests among these institutional settings, or "situses" as Bell terms them, and hence the formation of a coherent class here is unlikely. Second, Bell argues, cultural and political attitudes often flow not from economic interests but from ethnicity, religion, and family ties. Finally, he suggests that the new class "is a mentality not a class" (p. 186). The ideology which so offends the neoconservatives has emerged from the coexistence of an expanded state apparatus and a hedonistic culture. It is not the preserve of one class.

Reading this book one finds a lot of polemic writing, pages of armchair theorizing, and some data. My sympathies lie with Bell's assessment that the new class is a muddled concept. However, the essays are well written and mercifully short, so the volume serves as a useful introduction to the subject.

Leadership. By James MacGregor Burns. New York: Harper & Row, 1978. Pp. ix+530. \$15.00.

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James MacGregor Burns is a distinguished political scientist (at Williams College), sometime activist, teacher, and winner of both the Pulitzer Prize and the National Book Award. He has written widely on political leaders and leadership. In my view, *Leadership* is his chef d'oeuvre, the culmination of a lifetime of research and reflection; in a word, a masterpiece.

What Burns sets out to do in this book is to reconceive the exquisitely complex field of leadership, crossing disciplinary boundaries that have separated study of Freudian intrapsychic deposits from sociological/Marxist systemic constraints or the contemporary political leadership scene from ancient regimes. He also takes us across cultures: Asian, European, African, and old-fashioned raw American political leadership. He manages this extraordinary feat by stretching his field wider and frustratingly wider and then bringing it together with a refined and directed focus, thus ameliorating the ambiguity which the topic of leadership always generates.

This book is a masterpiece because it fulfills these three criteria of quality: significance, competence, and likability. It is significant because it presents the first truly interdisciplinary and general theory of leadership (and what leadership is not) that can be applied to political, industrial, and informal organizations, that has implications for small groups and macro-institutions, and that fuses, beyond anything in the leadership literature, theory and practice. It is competent in that the author usually stays within the boundaries of accepted research and theory (when he strays, he strays

gently and inquiringly). His knowledge is encyclopedic: history, philosophy, political science, of course, and he has also managed to assimilate and communicate the widest possible range of relevant literature.

Leadership is likable because it is a gracefully written book, a work of a deeply nuanced and cultivated mind, and most of all, a contribution that consistently manages to avoid the three banes of most of the literature on leadership: the obvious, the irrelevant, and the absurd.

Leadership is organized into five sections. Part 1, a general introduction to the topic, includes some rough definitions, a presentation of what is and is not to follow, and a rough outline of the author's concepts of "transactional" and of "transforming" leadership. Part 2 is titled "Origins of Leadership." Using the leadership lives of Gandhi, Lenin, Hitler, and Wilson as "representative anecdotes," it recreates and brings to life the newest findings of personality, sociological, and political theories, thus grounding the reader's perspective of leadership in a vast and complex conceptual area. Part 3 is about "transforming leadership," the province of leaders who, in the author's words,

shape and alter and elevate the motives and values and goals of followers through the vital *teaching* role of leadership. . . . The premise of this leadership is that, whatever the separate interests persons might hold, they are presently or potentially united in the pursuit of "higher" goals, the realization of which is tested by the achievement of significant change that represents the collective or pooled interests of leaders and followers. . . .

Transformational leadership is more concerned with *end-values*, such as liberty, justice, equality. Transformational leaders "raise" their followers up through levels of morality, though insufficient attention to means can corrupt the ends. [Pp. 425-26]

Burns elaborates on and embellishes the concept of transformational leadership with separate chapters entitled "Intellectual Leadership," "Reform Leadership," "Revolutionary Leadership," and, finally, "Heroes and Ideologues."

"Transactional Leadership" is covered in part 4 with separate chapters entitled "Opinion Leadership," "Group Leadership," "Party Leadership," "Legislative Leadership," and "Executive Leadership." Burns defines transactional leadership as a "reciprocal process of mobilizing, by persons with certain motives and values, various economic, political, and other resources, in a context of competition and conflict, in order to realize goals independently or mutually held by both leaders and followers. . . . The object in these cases is not a joint effort for persons with common aims acting for the collective interests of followers but a bargain to aid the individual interests of persons or groups going their separate ways" (p. 425).

Part 5, "Implications: Theory and Practice," consists of three chapters, "Decision and Change," "Toward a General Theory," and "Political Leadership as Practical Influence," and is a brilliant epiphany that brings together and finally answers all the questions.

Burns's contribution is this: he takes into account all the complexities of leadership—biological, social, cognitive, affective, and circumstantial (“structures of opportunity and closure around it, that it may emerge at different stages in different peoples’ lives, that . . . it manifests itself in a variety of processes and arenas” [pp. 427–28]) and then goes on to generalize about it.

What are his generalizations? Leadership is collective; there is a symbiotic relationship between leaders and followers, and what makes it collective is the subtle interplay between followers’ needs and wants and the leaders’ capacities to understand, one way or another, these collective aspirations. Leadership is dissensual: that is, without conflict (peacefully managed), we would all be trapped in a false utopian dream. Leadership is “causative,” meaning that—and here is where Burns makes one of his most stunning contributions—leadership can invent and create institutions (or ideas or documents or even memories) that can empower followers to satisfy their needs. Here, Burns succeeds in avoiding a common false dichotomy in writings on leadership: that of the “great man” theory and the “situationalist” theories. Finally, according to Burns, leadership is morally purposeful and elevating. Which means that leadership, if nothing else, chooses purposes that are based on key values of the followers and creates structures that embody them. It also means that leaders can, through their skills, move followers to higher forms of consciousness, such as liberty, freedom, and justice. For Burns, Gandhi most fully embodies this aspect of leadership. Perhaps Wilson, though, most nobly embodies Burns’s overall vision of leadership: “By boldly interpreting the nation’s conscience, [he] could lift a people out of their everyday selves. That people can be lifted *into* their better selves is the secret of transforming leadership and the moral and practical theme of this work” (p. 462).

I should add that the book is not perfect, that it contains (I should say that the imperfections inhere in the book’s thesis) some difficulties. For one thing (the lesser of two key problems), Burns seems unable to resist telling everything he knows.

More important than this cavil, by far, is Burns’s moral imperative, which comes through in his explicit preference for Gandhi and Wilson, his concern for moral uplift, his reliance on the still scanty empirical backing for the work of Abraham Maslow and Lawrence Kohlberg, and his selection of studies which confirm his own metaphysical pathos.

But then again, who can tackle the topic of leadership with all of its practical and theoretical complexity and not import his own morality and value system into his work? And it just may be that Burns’s theoretical breakthrough contains the solution to the mystery of leadership in a democratic society.

Time will tell. And it already has. I have to confess that this book review is two years late. It is late for any number of reasons, not the least of which is that this book is a pleasure to read and very difficult to review. An analogy is a question once asked of high school seniors: “Describe the

universe and give three examples." Well, James MacGregor Burns's *Leadership* not only stands up well but, like great wine, ages beautifully. It is a book which every social scientist and citizen-student of power must read.

The Structure of Professionalism: A Quantitative Examination. By John B. Cullen. New York: Petrocelli Books, 1978. Pp. xiii+290. \$15.00.

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Boston University

The sociology of the professions has experienced something of a renaissance in the past decade. A substantial body of literature has appeared, raising important critical questions concerning professionals' claims to higher rewards and autonomy because they are experts performing essential services for the good of society. The new literature largely argues that professions are constructed by occupational position holders seeking to enhance their market position. Some authors, notably Magali Sarfatti Larson (*The Rise of Professionalism* [Berkeley and Los Angeles: University of California Press, 1977]) have begun to develop sophisticated theories about the conditions in the class structure of advanced capitalist societies supporting professionalization efforts.

This tradition, drawing mostly on the Weberian power-conflict framework, stands in sharp disagreement with the technical functionalist school of the 1950s represented by the work of Kingsley Davis and Wilbert E. Moore and of W. J. Goode. This approach claims that professions are functionally necessary institutions, whose power and prestige are accorded them by "society" in exchange for the scarce knowledge and skills which professions develop, master, and utilize for the general social good.

John B. Cullen's *The Structure of Professionalism* is an effort to address some of the central issues that have arisen in this debate. Cullen offers an empirical argument to support his contention that both technical functionalist and power theories have some merit. Fortunately, Cullen does not follow the technical functionalists' fruitless effort to ascertain why some occupations are called professions and some are not. Instead, he states, "Professionalism . . . is primarily concerned with the stratification of occupational groups" (p. 22). His focus, then, is on the sources of the various attributes of professionalism, and their impact on the distribution of rewards among occupations.

Cullen's thesis can be stated in two major propositions: (1) "occupational task-complexity and intellectual sophistication are the major single determinants of professionalism's characteristics"; and (2) "to the degree that professionalism's characteristics are not determined by task complexity/intellectual sophistication, they are determined primarily by the occupational group's power" (p. 204).

Cullen's argument for the first proposition draws on a *Dictionary of Occupational Titles* scale analyzing the actual job requirements of a number of different occupations. Cullen shows that jobs with the greatest task complexity and intellectual sophistication are the likeliest to be held by the most highly educated, to be the most remunerated, and to have the highest level of associational activity. Cullen concludes that "major differences in occupational incomes *legitimately* arise because occupations *require* different degrees of intellectuality for adequate performance . . ." (p. 185, my italics). This finding, Cullen correctly concludes, is consistent with the technical functionalist theory of professionalism.

On the other hand, Cullen's second major proposition is that once an occupation requires a certain level of technical complexity and intellectual sophistication, professional associations can utilize these attributes as power resources to achieve greater rewards. In one interesting section, Cullen suggests that the principal object of professional manipulation is higher education, where professional groups attempt to influence the placement of their curriculum within the framework of the university. Cullen shows that occupations which are highly organized receive greater income rewards than occupations of the same level of complexity and sophistication that do not organize associations. This finding, he notes, is consistent with the power theory of professionalism.

Cullen's argument is flawed by two major problems. First, he insists that the only way to move the functionalist/power theory debate forward is to test their propositions empirically. But Cullen's methodological analysis contains so many theoretical assumptions that his data analysis simply confirms the framework he employs. In the end, the reader is still forced to question the underlying theoretical assumptions of the book and must dig beneath the surface appearance of rationality and scientism created by scales, multiple regression tables, and significance tests.

The second major problem with Cullen's argument is the theoretical framework itself. Cullen's central assumption is that technical complexity and intellectual sophistication are legitimate and "intrinsic" (p. 64) attributes of occupational success, while professional associations are political structures created to enhance an occupation's market position in competition with other groups. Cullen's argument has the merit of challenging some power theorists' simplistic view that professionals' claims to possess particular skills and knowledge are little but reflections of their power. But Cullen does not even acknowledge the more important efforts to show that one of the most important manifestations of professional power is precisely the capacity to elaborate and monopolize complex knowledge primarily to maintain or enhance market position. Instead, he simply asserts that all technical knowledge results from some unstated process of development separate from a group's interests. In short, Cullen adopts the mainstay of the technical functionalist position—treating technique as an independent variable—without adequate theoretical discussion. And, because he does

not take up this question rigorously, Cullen's effort to argue a reconciliation of the technical functionalist and power theories is not convincing.

Science and the Sociology of Knowledge. By Michael Mulkay. Winchester, Mass.: Allen & Unwin, 1979. Pp. 132. \$21.00 (cloth); \$8.95 (paper).

Sal Restivo

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In the past decade, discussions of the Mertonian sociology of science have led to the creation of a variety of Marxist, conflict, and confluence theories of science and stimulated a renaissance in the sociology of scientific and mathematical knowledge. Michael Mulkay's *Science and the Sociology of Knowledge* is a well-written, concise, and stimulating introduction to an important "revision" of the "customary view" in the sociology of science. Mulkay himself has helped to shape this revision, in part by drawing attention to scientific discourse. He argues that the "new" philosophers, sociologists, and historians of science (e.g., N. Hanson, M. Hesse; H. Collins, J. Law, and S. Woolgar; and T. Kuhn) have converged on an account of science as a social enterprise of interpretation and construction. This book is one of the best introductions to recent developments in the sociology of science for nonspecialists (including university students) I know of. As for specialists, I think they will find the book engaging and challenging; it is an articulate statement that will invite affirmations from the faithful and constructive criticism from the opposition. My own reservations about the book stem from its "revisionist" orientation.

Durkheim, Marx, Mannheim, and W. Stark, Mulkay notes, tend to agree that science is a social activity but scientific knowledge is independent of social contexts. This is the customary view in the sociology of science. Mulkay points out that Stark is more doctrinaire than Mannheim in exempting scientific knowledge from sociological inquiry (even though Mannheim's statement on the recalcitrance of " $2 \times 2 = 4$ " seems to me to set unequivocally the limits of his sociology of knowledge); and he is quick to note that Marx's writings support the view that science is a social phenomenon and the notion that science is distinct from ideology. The latter view is especially evident in Marx's conceptual analysis of the differential calculus. Mulkay does not comment on Marx's distinction between science-as-it-is and "human science." This omission symbolizes the fact that while Mulkay can conceive an alternative interpretation of how modern science works, he cannot conceive an alternative to modern science.

Mulkay's new view of nature relies too heavily on scholars whose allegedly heretical notions disguise a commitment to modern science and its social apparatus as the paradigmatic mode of discovery and explanation. Still, he does give a new account of nature and science: uniformity is not an aspect of reality but an artifact of scientific accounts of reality; facts

are theory dependent and unstable in meaning; observation is an active, interpretive process; and knowledge claims are not assessed according to a set of invariant, universalistic criteria: they are negotiated. Mulkay concludes that physical reality constrains but does not uniquely determine the conclusions of scientists (from which it seems to follow that Mulkay's conclusions are not uniquely determined by social reality). He does not discuss the subtle issues involved in considering the relationship between a recalcitrant reality and the human capacity for creating and elaborating "meanings"; but this is an unresolved problem in the sociology of science.

Mulkay argues that scientific knowledge is a product of cultural contingencies. But his stress on science as an autonomous research community, and his demarcation of physical and social realities, attenuates his contingencies thesis. When he turns his attention to the societal context of science, he does so armed with the "customary" idea of internal and external aspects of, and influences on, science (an idea some sociologists of science are intent on discarding) and under the guidance of Durkheim's contention that modern science is becoming increasingly self-sufficient. By arguing that it is still possible to identify external influences on science, Mulkay leaves the impression that science could eventually be transformed into the autonomous social system anticipated by the functionalists. Curiously, he affirms Gerald Holton's remark that scientific discovery is "as varied as the temperament of the scientists"; this seems to me to vaporize the sociology in the sociology of science. When we join this affirmation to Mulkay's argument that physical reality does not uniquely determine scientific knowledge we come perilously close to an image of science as a random behavioral repertoire operating on a random opportunity system, and sociological nihilism.

Mulkay's revisionism, with its stress on negotiation, is a form of neo-functionalism which has no central place for alienation, elitism, exploitation, and conflict. Indeed, his discussion of scientific culture as a pluralistic arena of shared interpretive positions based on flexible symbolic resources is almost Parsonian. Mulkay apparently still believes in the myth of the Kuhnian revolution in the sociology of science. But the Kuhnian and Mertonian "paradigms" are quite compatible. For a radical alternative to functionalism, we must turn to world view analyses. Oswald Spengler pioneered in the world view analysis of scientific and mathematical facts. The world view approach developed by such metascientists as Clifford Hooker, Paul Feyerabend, and David Bohm leads to sociologies of science in which context and cognition are radically inseparable, contingencies are conceived to be constitutive of knowledge, the focus is on "local historicity" (to borrow Harold Garfinkel's term) rather than on the mythical "scientific community," and struggles for power and privilege come into view as central features of the intellectual strategy we call "research." While Mulkay is clearly sympathetic to some of these ideas, his revisionist orientation inhibits his attempt to formulate a full-fledged alternative to the Mertonian-Kuhnian approach.

Genesis and Development of a Scientific Fact. By Ludwik Fleck. Edited by Thaddeus J. Trenn and Robert K. Merton. Translated by Fred Bradley and Thaddeus J. Trenn. Foreword by Thomas S. Kuhn. Chicago: University of Chicago Press, 1979. Pp. xxviii+203. \$17.50.

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University of Pennsylvania

Ludwik Fleck was a Jew born in Poland in 1896, trained in medicine at Lvov University, imprisoned by the Nazis, and a witness at the Nuremberg trials. He died in Israel in 1961. During the course of a distinguished research career he worked in serology, hematology, experimental medicine, immunology, and bacteriology and published extensively in German, Polish, French, English, and Russian. He also wrote on the sociology of science, publishing, between 1934 and 1947, four articles in Polish as well as *Genesis and Development of a Scientific Fact*, a study of the emergence of the modern concept of syphilis, which was issued in German by a Swiss publisher in 1935. The book was not widely reviewed and few copies were sold—roughly 200 of an edition of 640. Under the sponsorship of Robert Merton and Thomas Kuhn, it has now been translated and reissued. Fleck's promoters argue that his ideas anticipated trends in the history, philosophy, and sociology of science that converged with the publication of Kuhn's *Structure of Scientific Revolutions* in 1962. Indeed, Fleck's book may constitute a catalytic agent in this intellectual process, for Kuhn happened upon it at a critical point of his career as he was moving from physics to the history of science.

According to Fleck, the work of scientists can never be described as unmediated encounters with natural phenomena. Scientists' perceptions are structured by both the general culture and the habits of mind and behavior peculiar to their community—Fleck's "thought collective"; scientific habits are no less products of sociohistorical experience because they are localized in a professional subculture. The process by which the disease entity we now call syphilis came to be specified and its diagnostic measures refined exemplifies scientific development. The moral and emotional associations of syphilis exaggerate the influence of social factors on its scientific treatment, but all scientific work is grounded in its own social context. Sociologists have erred in granting privileged status to scientific knowledge by assuming it to represent timeless truth, unique among cultural products. It is "psychologically impossible," says Fleck, for scientists' perceptions to be uncontaminated by social conditioning. Furthermore, if we define good science by its results, we see that it frequently derives from untenable assumptions and irreproducible experiments, rather than from the strict logic of purely technical expertise.

Scientific findings are not mere epiphenomena of cultural bias, however. Fleck distinguishes between "active" and "passive" elements of knowledge,

the latter reflecting natural regularities. If, for example, assigning the number 16 to the atomic weight of oxygen is arbitrary, it nevertheless follows from this convention that the atomic weight of hydrogen must be 1.008. "In our history of syphilis the combination of all venereal diseases under the generic concept of carnal scourge was thus an *active association of the phenomena*, explained in terms of cultural history. In contrast, a restriction of the curative effect of mercury . . . represents a *passive association* with respect to the act of cognition" (p. 10). The development of the Wasserman reaction as a test for syphilis realized the traditional concept of syphilitic blood, but the use of this technique entailed the redefinition of the stages and types of the disease, as well as rejection of previous notions of its relations to other diseases.

The subculture of scientific thought collectives has distinctive features. Acknowledging the influence of Durkheim and the gestalt psychologists, among others, Fleck defines the thought collective as an entity sui generis, irreducible to the sum of its component parts. Its research findings are shaped by its material culture—its esoteric techniques and equipment—as well as by theoretical commitments. "Every comprehensive theory passes first through a classical stage, when only those facts are recognized which conform to it exactly, and then through a stage with complications, when the exceptions begin to come forward" (pp. 28–29). It may appear that new theories permit some of the same findings as the old, but the significance of these perennial "facts" will be altogether different. To some degree, this temporal cycle of scientific certainty has a static, sociometric correlate in the structure of the scientific community; scientific knowledge is presented with diminishing degrees of simplicity and certainty as the audience for it shifts from the general to the exoteric and, then, to the esoteric circles of science. Esoteric science is reported in professional journals in personal and provisional terms, which accurately reflect its practice, for it rests largely on tacit knowledge—the ability to produce desired results, which can only be learned from personal contact with a successful scientist, rather than replicated from procedural formulae.

By pointing to past and present fashions in the study of science, we can explain Fleck's long neglect and current appeal, as well as predict that some aspects of his work are not likely to be imitated. Thomas Kuhn's account of scientific change has clearly been the most influential work in science studies for the past two decades, however much its interpreters have been denounced by its author. Fleck's cycles of scientific thought are not revolutionary, but they are reminiscent of Kuhn's. Far more pronounced are the affinities between Fleck's ideas and those current among many British and Continental sociologists of science. They, too, have rejected the notion that scientific knowledge is culture free, the axiom at the base of the American tradition of sociology of science. They agree with Fleck that it is not because scientists adhere to the Mertonian norm of "disinterestedness" that research progresses, but the reverse. These sociologists might profit, however, were they to adopt Fleck's distinction between active and

passive elements of knowledge, for it offers one solution to the problems posed by their uncompromising relativism. And many of the relativists favor "anthropological" studies, based on participant observation of laboratory behavior. As a working scientist, Fleck was able to function as an anthropological observer, and like his contemporary analogues he reported that tacit knowledge and contingent decisions were essential to research. Finally, many contemporary European sociologists also assume the collective character of scientific work; this assumption makes analysis of the motives and perceptions of the individual agent nonsensical. This rejection of methodological individualism is likely to prove unacceptable to American sociologists. Despite American graduate students' ritual indoctrination in Durkheim's sociologism, and despite occasional vogues for examination of "contextual" or "structural" effects, most American sociologists have unfortunately assumed that rigorous scholarly standards require statistical nominalism.

One cannot simply conclude, however, that Fleck's ideas have not previously won a large audience because it is only recently that sympathetic intellectual communities have come into being. The vicissitudes of Fleck's life alone provide a partial explanation. But one must also look to his book itself. Much of it is devoted to medical arcana, barely translated for the layman. Persistent readers will be rewarded with many perceptive observations, but they will have to abstract them from an often disorganized text. The defects of the book well illustrate Fleck's thesis. It has been neglected not merely because there have been few like-minded scholars but also because it was the product of an intellectual isolate. Had Fleck functioned as the member of a thought collective, his ideas would have been tempered and rendered far more intelligible.

The Cancer Mission: Social Contexts of Biomedical Research. By Kenneth E. Studer and Daryl E. Chubin. Beverly Hills, Calif.: Sage Publications, 1980. Pp. 319. \$18.00 (cloth); \$8.95 (paper).

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Readers should be forewarned that, despite the title, the main concern of *The Cancer Mission* is not the politics of cancer and of medical research, but an equally controversial, and closely related, question, "Are ideas independent or dependent variables for the sociology of science?" (p. 6). The question is causing an uproar among sociologists of science. The main issue is how to study, and perhaps to predict and intervene in, scientific development. Scientists, policymakers, and critics of science have joined the fray, for ultimately the contention is over whether the scientific community is relatively autonomous or whether its work is (or should be) politically determined.

In their noteworthy and ambitious contribution to this debate, Kenneth Studer and Daryl Chubin argue that, in focusing on the social control of science, the sociology of science has failed "to formulate the question so that the relationship" (p. 236) between social structure and cognitive systems can be studied. The reason for this lacuna is that sociologists have avoided the content of science. Therefore, when sociologists of science use such well-established methods as citation and network analysis, they misrepresent both the organization and the work of the scientific community by treating the products of science as if they were indeed the processes of science. This has resulted in premature generalization concerning the dynamics of scientific development. Studer and Chubin are particularly critical of sociologists' wholehearted embrace of the Kuhnian concept of scientific revolution.

The Cancer Mission is a case study of the development of research on reverse transcriptase, an enzyme discovered in 1970. This discovery, by deepening scientific understanding of the mechanisms of cell replication and mutation, opened a new era in cancer and genetic research. Studer and Chubin examined the scientific literature on reverse transcriptase, analysed the career patterns of scientists working in this specialty area, and interviewed key researchers and National Cancer Institute administrators about the discovery. The authors' methodological discussion will be useful to those who have been either bewildered by or skeptical of citation analysis and related quantitative tools peculiar to the sociology of science.

On the basis of their case study, Studer and Chubin propose the "confluence theory" of scientific development, which postulates that advances in biomedical research result from the convergence of diverse lines of inquiry onto a single problem. The resulting scientific grouping is short lived and does not give birth to a new field; therefore biomedical research networks are fluid, continually changing in response to new scientific findings.

If I understand Studer and Chubin correctly, the noteworthy point is that breakthroughs in the biomedical sciences, although they may appear to be revolutionary paradigm shifts, are actually continuous. The appearance of discontinuity results from the assumption that biomedical subdisciplines are static when, in fact, they are composed of intellectual nomads who migrate from problem to problem. The confluence theory is a potentially fruitful approach to the biomedical sciences. However, the authors sometimes undermine their argument by making straw men of their opponents. One glaring example is the assertion that "normal science" is routinely used as a pejorative term (p. 55).

The major weakness of *The Cancer Mission* is that such a wealth of information and ideas is presented that it is difficult to follow the argument. The book draws from and adds to three distinct bodies of literature: (1) the sociology of scientific knowledge, (2) the recent history of "the war on cancer," and (3) the history of tumor virology in general and reverse transcriptase in particular. Integrating all this is a formidable task; the authors are to be applauded for their courage. Unfortunately, Studer and Chubin

assume too much knowledge on the part of the reader; they have not organized the material to meet the needs of the diverse audience the book will (and should) attract. Particularly confusing is the extensive use of block quotes from the scientific literature and from interviews with scientists; the reader has to struggle to figure out what the point is. For example, there is no simple explanation of just what, exactly, reverse transcriptase is, and why it was so important, prior to the authors' launching into a detailed account of its history. The puzzled reader would do well to consult *Cancer: Science and Society* by John Cairns (San Francisco: W. H. Freeman, 1979).

In closing, I want to emphasize that the problem of organizing and presenting technical material is not peculiar to *The Cancer Mission*; it is a challenge inherent in the new view of the sociology of science. If sociologists are to address the content as well as the forms of scientific research, they must become competent science writers. The ability to talk with scientists about their work is not enough. If the general sociological reader, all too often science shy, is not addressed, we face the prospect that the sociology of science will be fragmented into highly specialized subdisciplines accessible only to those with prior training in the area.

Ethical Dilemmas and Social Science Research. By Paul Davidson Reynolds. San Francisco: Jossey-Bass, Inc., 1979. Pp. xx+505. \$19.95.

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Ethical Dilemmas and Social Science Research is a welcome addition to a growing resource network upon which social and behavioral scientists can draw in order to negotiate the research minefields of the 1980s. Rather than moving into obscurity as some have predicted, social science research is more and more being held accountable for its applications and consequences. Paul Reynolds's book is a scholarly, detailed, and full account of highly pertinent moral and ethical issues and of strategies for resolving them.

Three general dilemmas form the organizational core of the book. These are: (1) the research dilemma of proceeding with studies which are socially beneficial yet which pose risks to the rights and welfare of individuals; (2) the scientist-citizen dilemma arising out of contradictions between the roles of objective scientist and responsible citizen; and (3) the application-of-knowledge dilemma which occurs when scientific knowledge is used in ways that violate the values of the scientist.

Reynolds tackles the dilemmas head on by a historical discussion of the standards to be followed if one is to be moral, good, or ethical. Fortunately, there is no sanctimoniousness here in the sense of his telling us how we should resolve the dilemmas. Instead, the reader is given a rich historical

context and the detail necessary to form his or her own personally acceptable solutions.

Part 1 deals with the research dilemma in terms of informed consent, cost-benefit thinking, and deception, all within laboratory experiments, field experiments, observational/participant observational designs, and archival research.

Part 2, in dealing with strategies for resolving the research dilemma, focuses on professional associations (codes of ethics), governmental institutional review board (IRB) rules and regulations, and laws affecting the rights of researchers and subjects. Reynolds is critical of professional codes of ethics for nonenforcement of standards and his review of IRB functions is very thorough and informative.

Part 3 is devoted to the application of social scientific knowledge and the scientist's relationship to society. The roles of autonomous scientist, social activist, social critic, and applied scientist are analyzed relative to their respective rights and obligations. Many examples of the uses and misuses of research findings provide the substance of the discussion of the dilemma between an investigator's professional and civic values. This is fascinating material for any reflective scientist.

Also, the six appendixes that contain codes of ethics such as the Nuremberg Code and composite social science codes dealing with investigators' responsibilities save the interested reader considerable time in finding the major social science oriented codes.

In concluding, let me say that the substance of Reynolds's book is not altered by the most recent IRB rules or by regulations of the Department of Health and Human Services. All scientists should have this book on hand to refer to, at the very least, during the design phase of research. It is strong medicine but a much needed treatment which promotes greater accountability and social utility of the social and behavioral sciences.

New Directions in American Intellectual History. Edited by John Higham and Paul K. Conkin. Baltimore: Johns Hopkins University Press, 1979. Pp. xix+245. \$16.00.

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It is often hard to figure out how best to assess collections of essays which assay the state of an academic art and seek to point it in "new directions." This is especially the case with history, which, as a whole or within most of its subfields, has no governing *problematic* and which is so eclectic (some might say cavalier) in the melange of methods and theories it appropriates from other disciplines. Few of the many manifestos heralding new

departures appear to have had much discernible effect on the actual practice of history. With its variety and eclecticism, history does not easily lend itself to blueprints: even when it is most methodologically and theoretically self-conscious, the best history usually derives much of its persuasiveness from the combined particularism of a uniquely construed subject and a given historian's idiosyncratic mix of insights and approaches. Subsequent works that take such "path-breaking" studies programmatically and, as is often the case in more formalized disciplines, seek to replicate them are rarely of much more than informational interest.

It thus seems to make sense not to assess *New Directions in American Intellectual History* as prescription but to approach it as a text which locates a particular style of historical inquiry within its own traditions and general discipline, as well as within a broader intellectual context. Editors John Higham and Paul K. Conkin have compiled a book that is well suited for this. In a first section directed to "definitions," Laurence Veysey considers the character and role of intellectual history in the light of the new social history, that branch of history that has seemed recently to be at the cutting edge of the discipline, Gordon Wood analyzes its relationship to social science more generally, and David Hollinger and Rush Welter examine two concepts—"discourse" and "the national mind"—that have informed much of the work in American intellectual history. This section is followed by two groups of essays which focus on the two ends of the spectrum of intellectual history, formal ideas and popular culture, by addressing either a conceptual issue or a historical phenomenon that seems to illuminate the character and dimensions of the field. Thomas Haskell examines the deterministic implications of the way in which some of the new directions approach ideas. Murray Murphey considers how the place of beliefs in modern bureaucratic society requires new ways of conceiving culture as an integrating system. Sacvan Bercovitch, Henry May, and Dorothy Ross examine religious or political traditions. Neil Harris, looking at the impact of halftone photography, and David Hall, looking at the world of print in 17th-century New England, suggest how consideration of the technologies and forms in which images and ideas are conveyed enhances our understanding of both the character and the content of belief and knowledge in society. Thomas Bender examines the local community and the professions as the structures shaping patterns of thought in 19th-century America, while Warren Susman zeros in on the emergence of "personality" as a central cultural construct in 20th-century America.

Unlike many collections of essays even on a common theme, this volume is perhaps best read as a whole (although some of the essays can be profitably read on their own). Partly because of the range and diversity of the essays, it succeeds in giving a broad sense of the direction in which intellectual history appears to be going. Moreover, what unifies the volume and makes it such an interesting mapping of the field is that as a whole it embodies a new way of construing the materials, agenda, and scope of intellectual history, a way of thinking that in large measure derives from the

encounter of these and other historians in their own historical work with the writings of Michel Foucault, Thomas Kuhn, Kenneth Burke, Clifford Geertz, Quentin Skinner, and Victor Turner, to name just a few of the most prominent and often cited thinkers. The extent of this encounter can be seen not only in the general definitional essays and in the specific citations in all the pieces but also in how Bender, Harris, and Hall, for example, construe their specific topics. This is not to say that the volume seeks to promulgate a new orthodoxy or provide a blueprint, but that it displays the "discourse," to use a Foucauldian notion often cited in the book, the field of force, so to speak, within which much of intellectual history now operates. Typically, the authors have taken these thinkers eclectically rather than systematically, not as sources for a theory that can be turned into a research design and then applied to a controlled body of data to "explain" a particular historical phenomenon, but as a source for the insights by which to construe and analyze thought as a historical phenomenon.

The general orientation or new direction that seems to have come from this encounter centers on the notion that intellectual history is ultimately concerned with meaning and language, that its materials consist of expressive acts which, to be understood historically, need to be examined in the particular ideational and institutional contexts in which they occur and operate. As the essays reveal, this orientation to the field has several consequences. It puts to rest the tiresome debate over whether "ideas" or "social forces" are the essential causal agents of history and, more importantly, suggests how the essential mode of understanding in intellectual history—and hence its analytical procedures and styles of argumentation—is essentially descriptive rather than formally explanatory. (This, I think, helps account for the enormous resonance Clifford Geertz's notion of "thick description" has had for many intellectual historians.) Moreover, it extends the scope of intellectual history and alters its approach to the formal bodies of thought that traditionally have been its principal focus, by recasting and expanding the notion of the "text"—the essential datum of intellectual history. It construes a text, whether in verbal, visual, or dramaturgical form, and whether a letter, a song, a treatise, a dance, a ceremony, a painting, or a photograph, as a complex expressive and social act, an exchange, as it were, between actor and audience, whether immediate and known or remote in time or space and anonymous, which operates within and through a received vocabulary of images, ideas, and conventions. Finally, the new orientation places the materials and subject matter of intellectual history at the center of social and cultural process, making, in Wood's terms, "social and intellectual history . . . inseparable" (p. 32).

Such a brief and compressed description of *New Directions in American Intellectual History* cannot do critical justice to the diverse ideas and discussions contained in the book. But perhaps it does convey something of the direction of the enterprise to social inquirers, many of whom are also coming to be situated, though in quite different ways, in the broad discourse that has had such a transforming influence on intellectual history.

Technology and Society under Lenin and Stalin: Origins of the Soviet Technical Intelligentsia, 1917-1941. By Kendall E. Bailes. Princeton, N.J.: Princeton University Press, 1978. Pp. xiii+472. \$30.00 (cloth); \$12.50 (paper).

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The technical intelligentsia is the most influential stratum in contemporary Soviet society: by the mid-sixties 65% of the full membership of the Central Committee, by the early seventies about 80% of the membership of the Politburo belonged to the group Bailes defined, by its higher technical education, as "technical intelligentsia." *Technology and Society under Lenin and Stalin* is an impressive attempt to account for the formation of the Soviet techni-intelligentsia during the years between the October Revolution and the outbreak of the Second World War.

The rise to power of a substratum of intellectuals in a society which claims to be the dictatorship of the proletariat is necessarily full of contradictions and paradoxes. Especially with Stalin's ascendancy to the top of the Soviet power hierarchy an anti-intellectual ideology seems to have become dominant for a while. Bailes's central thesis is that despite persecutions against some of its members and despite its seeming marginality "the Soviet technical intelligentsia was more active than previously thought in changing the social structure of the Soviet Union and the cultural values of its people in this period" (p. 8).

While developing this thesis, Bailes casts doubts on the accuracy of the ideological self-perception of the Soviet society as "dictatorship of the proletariat," but he also challenges the dominant Western views of the nature of the Soviet Union. Bailes does not find either the "totalitarianism" or the group-conflict theory an accurate enough description of social processes in the contemporary Soviet Union and he suggests a new "interactional model." "The . . . Communist systems . . . can be better understood by analyzing the interaction between the forces that tend to hold groups together for cohesive action and forces of social conflict that tend toward their dissolution or transformation. . . . The real problem for the historian of Soviet society . . . is to identify more precisely the composition of these forces . . ." (p. 8).

While working with this "interactional model" Bailes seems to be in a better position than those who subscribe either to the totalitarianism or the group-conflict theory to explain why and how the Soviet types of societies are able to reproduce themselves. With this interactional model Bailes is able to identify not only the coercive but also some "voluntary" forces of cohesion. In this fascinating history of the making of the Soviet technical intelligentsia we can understand how some of the interests of the technical specialists and the party bureaucrats began to merge. This merger, after

the death of Stalin, would result in the increasing overlap of the bureaucrats and the technical intelligentsia. But this did not happen just because the technical experts were coerced to obey the party: the very role the technical intelligentsia performed served the legitimacy needs of the party. The Bolsheviks "claimed legitimacy not only as the avant-garde of the industrial proletariat, but on their ability to transform nature and society. Until they did so, however, this basis for their legitimacy was in doubt, and they had to rely on a high level of coercion against those elements who viewed their power as illegitimate" (p. 383). A competent technical intelligentsia could help solve the legitimation problems of the party and in fact it is quite possible that Bolshevism could not, at any stage, achieve legitimacy without sharing some of the powers of the party bureaucracy with the "technocrats." Despite their mutual suspicions and conflicts the Bolshevik party bureaucrats and the technical intelligentsia depended on each other. The technical intelligentsia began to share the central political power only in the late fifties, but in many respects the earlier epochs prepared them to perform this role.

Even Lenin was not so "anti-intellectual" as some of his interpreters would like to present him. As Bailes notes, "Lenin remained a traditional Russian intellectual . . . [and] after 1917 he defended many interests of the intelligentsia against attacks from . . . workers. . . . Lenin was especially well disposed toward intellectuals who sought knowledge as a guide to action and this . . . predisposed him to view favorably the scientific-technical intelligentsia" (p. 47). Lenin regarded science and technology as pillars of the new society (p. 48). Already "by March of 1918 Lenin had moved to curb workers' control . . . a concern uppermost in the minds of most technical experts at that time" (p. 50). The technical specialist gained more power and higher rewards. At the Eighth Congress in March 1919 Lenin himself justified paying technical experts salaries that were often five or six times higher than those of skilled workers. Lenin in a way already had foreseen the coming merger of the technical intelligentsia with the party bureaucracy. In his speech to the Eighth Congress, Lenin expressed his hopes that with the consolidation of Bolshevik power the technical intellectuals "will themselves be attracted to our *apparat* and make themselves a part of it" (p. 51).

Such an absorption of the technical intelligentsia into the party apparatus turned out to be a slow and quite painful process, which required to a large extent the elimination of the old, "bourgeois" technical experts and the production of a new generation of "Soviet technical intelligentsia." Between 1917 and 1928 the bourgeois technical experts cooperated with the Communist power, but they were unlikely to join the party and maintained a skeptical distance from the Bolsheviks. In 1928 fewer than one thousand members of the technical intelligentsia belonged to the party (p. 138).

Stalin launched a strong "anti-technocratic" campaign in 1928 which led to two major show trials, the so-called Shakhty Affair in 1928 and the "Industrial Party Trial" in 1930. Bailes offers an empirically rich and

highly informative description of both of these affairs. From these two case studies it is obvious that, in a sense, the antibourgeois specialist campaign was a reflection of internal struggles within the party (Stalin used his attack on the bourgeois technical experts to discredit some of his major opponents, first of all Bukharin, and in this struggle he was trying to make political capital out of the anti-intellectualism of the rank and file party members), but it is also clear that Stalin's policy toward the technical intelligentsia "cannot be dismissed as simply a form of paranoia" (p. 120). As Bailes documents it, Stalin was actually following a dual policy: "the repression of the old specialists, particularly those who had been associated with technocratic tendencies; and a rapid promotion in the 1930s of a new group who combined the two categories, the so-called Red Experts whose education and experience were intended to merge technical expertise and political loyalty" (p. 119-20).

Indeed during the last years of the 1920s the growth of party membership accelerated among the technical intelligentsia, especially among those with higher technical education (p. 138). As Bailes notes, "the campaign to win over the technical intelligentsia to the party climaxed in 1930, the same year that the terror against this group also reached its height" (p. 139). For Stalin this meant assurance of political loyalty from technical experts, but for the technical intelligentsia this meant a firmer grasp on political power. Bailes quotes a Soviet graduate engineer, the director of a new automotive plant in the 1930s, who supposedly said: "Bureaucrats have arisen from the workers and peasants. This is a uniformly harmful phenomenon with which one must struggle, using the methods of technical progress. Engineers, physicians, scientists—people of concrete knowledge—must move into governing posts . . ." (p. 140).

After the new Red Expert stratum had been created in 1935, Stalin radically redefined his attitude toward the technical intelligentsia. Whereas during the late 1920s he regarded the technical intelligentsia as the class enemy of the working class, now he looked at the new technical intelligentsia as belonging to the proletariat. During these years Stalin developed his own version of a "new working class" theory. Already, by 1935, Stalin had suggested that "the elimination of the distinction between mental labor and manual labor can be brought about only by raising the cultural and technical level of the working class to the level of engineers. . . ." (p. 316). In fact the idea that the Soviet working class would eventually be raised to the level of technical intelligentsia was adopted as party policy at the December 1935 Central Committee plenum (p. 316).

Bailes notes that, during the 1930s, "the technical intelligentsia became politicized in favor of the Communist party, the Communist party became in part 'technocratized' by the infusion of the new members" (p. 200). As early as 1936, parallel with the great purges, specialists began becoming party and police officials for the first time (p. 331). Beria, who was appointed as head of NKVD in 1938 and who was a former engineering student himself, began to recruit many technical specialists to the political

police. A decisive step had been made, therefore, toward a system in which the party and technical elites would increasingly overlap and interlock with each other.

The analysis of this "new system of overlapping and interlocking elites" (p. 201) is beyond the scope of this book. Bailes's story ends with 1941, and such a new system emerged only after the death of Stalin; but Bailes's book is certainly a major contribution to understanding the nature and dynamics of this new system of elites in the Soviet Union and possibly in other Soviet types of societies.

Bailes is cautious in drawing his theoretical conclusions. He asks the question, "Had the technostucture as a whole become part of the ruling class . . . ?" (p. 412). "The evidence of this study does not support such a conclusion," is his answer (p. 412). At the same time Bailes acknowledges: "If, some time in the future . . . mobility would cease, if material privileges, access to education and to power become more hereditary, then the Soviet society may once again face a sharpened crisis of legitimacy, with the chance of increased and more open class conflict" (p. 426).

Technology and Society under Lenin and Stalin is a delightful book. It is sound scholarship and also tells an exciting story with a great deal of theoretical and political relevance. This book is a must for those who have an interest in the Soviet Union, in "actually existing socialist societies," socialist theory, and theories of the "new class."

A Critique of Sociological Reasoning: An Essay in Philosophical Sociology. By Charles W. Smith. Totowa, N.J.: Rowman & Littlefield, 1979. Pp. xi+155. \$15.50 (cloth); \$7.95 (paper).

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Charles W. Smith introduces some interesting ideas in *A Critique of Sociological Reasoning* yet seems unable to do anything of consequence with them. The volume is subtitled *An Essay in Philosophical Sociology* and Smith describes the venture as seeking "to analyse the basic concepts and unifying assumptions of sociological reasoning" (p. 1), undoubtedly a serious and ambitious project of potential interest to many of us. Our interest soon begins to waver, however, when we read that while the physical sciences can use an "object frame of reference" where the "key dimensions are sensory," the social sciences must use an "intentional frame of reference" where the "key dimensions are intentional" (p. 6), since this all sounds so commonplace. The complex philosophical and methodological issues involved are glossed over, trivialized, or (in one of Smith's favorite phrases) simply "touched upon." The result is an unilluminating discussion, bordering on the vacuous. One footnote, for example, reads: "The

concept of 'units of analysis,' should not frighten anyone. It means just what it says and no more, namely, the units, or entities, used in analysing the phenomenon under examination" (p. 137)!

What this book boils down to is a somewhat superficial and misleading examination of the intentional frame of reference. In part 1 it is examined in terms of three "levels of social reality" (the "non-cognitive," "meanings," and "meaning systems") and four "intentional dimensions" ("gratification," "power," "utility/economic," and "order/ordering"). The rationale for this schema seems very loose and arbitrary to me, and hardly a satisfactory basis for tackling the prime objective of the study in parts 2 and 3, namely, "to analyse the cognitive structures which underlie the more familiar sociological concepts" (pp. 29-30). Smith's essential argument is that the intentional frame of reference comprises cognitive structures that function as Kantian categories of experience and are "part of social reality, not merely the sociologist's reality" (p. 78). This leads him to endorse the familiar conclusion that "the underlying concepts of sociology and the underlying concepts of everyday life share a common origin, namely, the categories of social experience" (p. 111). For some reason Smith describes this as "not a very popular sociological view" (p. 111) while, at the same time, pointing out that it is the view shared by Durkheim, Weber, and Mead and has much in common with the structuralist views of Lévi-Strauss and Piaget.

As far as I can tell Smith has no explicit or reliable methodology for distinguishing distinct categories of experience. In part 2 he focuses on the "notions of 'oneness' which govern the patterns of social contact giving rise to social groups and their associated meaning systems" (p. 30) as important examples of such categories. I find this an interesting idea, but Smith does little to clarify with any rigor how or why such notions of group unity deserve special status as categories of experience. Instead he generates an ideal-type typology of sociological approaches to the analysis of social groupings "in terms of the types of collectivities and meaning systems which are accepted as prototypes" (p. 32).

Smith distinguishes "(1) socio-cultural/peoples approaches, (2) social class/interest group approaches, and (3) social system/exchange network approaches" (p. 32). Prevailing themes here are (i) an intentional frame of reference necessarily involves meaning systems; yet (ii) meaning systems themselves are invariably interdependent with social groupings; so (iii) to understand the particular character of a sociological approach we need to consider both the meaning system and the type of social grouping it represents (hence the double-barreled labels in his typology), the link being the notion of group unity involved. Smith's discussion is aimed at analyzing "the underlying structure of sociological reasoning in general" (p. 32), but I do not think he comes anywhere close to accomplishing this. He uses his simple typology as if it were a substitute for careful philosophical and theoretical analysis, and his account of sociological reasoning seems bland and contrived. I remain skeptical of the way Smith classifies social

theorists, and his references are curiously selective and dated, as if collected by someone not thoroughly acquainted with the literature. His treatment of social class under his second approach, for example, is extremely weak and inconsequential, while in order to preserve some consistency in his third approach he has to dismiss Blau's latest work as "so much nonsense" (p. 139)!

The philosophical and methodological analysis of an intentional frame of reference for social theory is, I believe, of utmost importance. Unfortunately Smith adds little of value in this book to the results already obtained by Talcott Parsons, Jürgen Habermas, Anthony Giddens, Richard Bernstein, Peter Winch, Alvin Goldman, Georg Henrik von Wright, Gerard Radnitzky, and others. Indeed, although Smith mentions a few of these in his preface he neither takes them into account in the main body of his text nor measures up to the standards they have set. To my mind no useful purpose has been served by publishing Smith's manuscript in its present form.

Religion and Politics in Latin America: The Catholic Church in Venezuela and Colombia. By Daniel H. Levine. Princeton, N.J.: Princeton University Press, 1981. Pp. xii+342. \$22.50 (cloth); \$6.95 (paper).

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Daniel H. Levine, as associate professor of political science at the University of Michigan at Ann Arbor, is the editor of *Church and Politics in Latin America* (Beverly Hills, Calif.: Sage Publications, 1980) and the author of *Conflict and Political Change in Venezuela* (Princeton, N.J.: Princeton University Press, 1973). In the latter book, he analyzed the development of political legitimacy in Venezuela, taking into account the importance of oil as well as the social role of the Catholic church in that development, and he also looked at the impact of political change and social conflict on the church itself. In *Religion and Politics in Latin America*, he adopts a comparative frame of reference in order to include also neighboring Colombia, where the Catholic church is more powerful and more firmly established than in more secularized Venezuela. Here, he focuses all his attention directly on the relationship between religion and political change. In describing and analyzing the complex interaction between these dual variables in those two rather special Latin American countries, he uses the newly fashionable Weberian-phenomenological approach rather than the standard positivistic-evolutionary stance usually adopted by students of religious and political development in Latin America. Also, he neglects the importance of economics, of social class, and of military might, focusing unilaterally on what he calls the dynamic and dialectical balance between religion and politics. Although he insists that

he gives "full and equal weight to each" (p. 23), since the book is basically about the beliefs of bishops, religion emerges as the leading force and the principal motivation for action in day-to-day affairs. Levine endeavors to show how the bishops' vision of the church and their view of authority are the major factors that influence their commitment in favor of social and political change. It is probably inevitable that religion will appear as a key source of guiding concepts and principles if one starts off, as Levine does, with interviews with religious officials, who by training and function are predisposed to affirm that they go from religious ideals to sociopolitical involvement, rather than the other way around. Even if some bishops were primarily motivated by economic, social, or political interests, they would be extremely reluctant, especially when talking officially to a non-Catholic social scientist from the United States, to couch their discourse in terms other than religious. It is highly probable, however, that the religious officials interviewed by Levine, including the more conservative ones, are in reality influenced primarily by religious considerations, but it is methodologically weak to base this conclusion exclusively on formal interviews or on official pronouncements. In spite of this shortcoming, the book is well organized and highly readable. It is a very impressive report on the religious and political ideology of an important segment of the episcopate of Latin America. At a purely descriptive level, it tells us a lot about Venezuelan and Colombian bishops. One senses however that the author is somewhat taken in and fascinated by the prudent and moderate officials who run the church in these two countries and that his gains in insight came at the cost of nearly adopting the position of the bishops through his use of a subjective approach.

The book opens with some considerations on the nature of the relationship between religion and politics in general and attempts to show that political action is influenced by religion, even if in fact both vary together and continually impinge on one another. In chapter 2, some general perspectives on religion and politics in Latin America are put forward. Levine shows the diversity of Catholicism in that part of the world, which goes from the right-wing pre-Vatican II integralism of the "tradition, family, property" type, which supports some quite barbaric and authoritarian military regimes, through the centrist position of the Christian Democrats and of the vast majority of bishops who accept the Vatican II reforms, to the left-wing revolutionary position of liberation theologians and of Christians for Socialism, who seek to go beyond even the bold declarations of the Latin American bishops at Medellín. Chapter 3 gives a brief historical sketch of politics and religion in Venezuela and Colombia and describes the historic public concerns of the episcopate in these two countries.

A very enlightening effort at classification of ideologies of religious elites is produced in chapter 4, which builds on the well-known typologies of Ivan Vallier (politicians, papists, pastors, pluralists) and Thomas Sanders (reactionaries, conservatives, progressives, innovators). It goes beyond them by distinguishing between different types along two analytically dif-

ferent dimensions: on a religious axis, the author identifies two categories (ecclesiastical and ecclesial), and on a political axis, he proposes three different possibilities (traditional, activation, and activism). The six ensuing types offer a more sophisticated classificatory model than Vallier's and Sanders's typologies, which end by failing to differentiate between religious and political conservatism and between religious and political progressivism. Although they are not necessarily and inevitably linked in all cases, there is nevertheless a certain congruence between these two kinds of conservatism and between these two kinds of progressivism. In the next chapter, about the bishops' views on the church and on authority, Levine shows how the fact that some bishops stress collegiality and see the church as the "Pilgrim People of God" rather than as a rigidly structured hierarchical institution, leads them to have a more open attitude toward action aimed at transforming society. Chapter 6 then compares the bishops of Venezuela and of Colombia on issues pertaining to social and political action. The author notes, for example, that "Venezuelan bishops appear much less structural (as opposed to moral) in their views of the world, but much more willing to accept dialogue and cooperation with Marxists—an activity directed in fact to the transformation of that world" (p. 189), and he explains this difference as being due to Venezuela's greater pluralism, to the weak position of the Venezuelan church, and to the necessity for it to coexist with various powerful parties of the left, especially at the local, grass-roots level. In Colombia, where the church has historically been more powerful, and where the left is less developed but more radical, dialogue with Marxists is seen as less useful and important for church officials. Colombian bishops, because of the history of violence in their country, are more fearful of violence and more reluctant to abandon their "neutral" political stance, even if they are constantly being drawn into politics by the need to defend some of their socially involved priests and laity from right-wing attacks.

For Levine, there is a dialectical relationship between thought and action, between religion and political change. True, his dialectical approach is counterbalanced by a more idealist phenomenological perspective, which theoretically and methodologically impels him to proceed from internal religious rules to overt social behavior, from the inside out, as he says. A slight slant in the direction of idealism, as opposed to materialism or economic determinism, does not lead him, as some might expect, to a conservative acceptance of the Christian or neo-Christian ideals of right-wing and centrist Latin American Catholics, but rather to a certain sympathy for the progressive left-of-center Catholics who are now moving about in the ideological and political space that extends between liberation theology and the pastoral-evangelical ideal that emerged triumphant from the Puebla Conference of Latin American bishops. Levine neither rejects nor wholeheartedly embraces the more revolutionary ideals of some Latin American Catholics who are quite close to Marxism, but he seems to think that the newer socially minded pastoral action orientation of the church and its leaders, which encourages locally based communities and which has a clear prefer-

ence for the poor while avoiding too narrow an involvement in partisan political struggles, has a better chance from a practical point of view to be effective and to have major political repercussions in the years to come in the authoritarian regimes that now dominate Latin America.

In sum, a slight political bias in the direction of reformism and an epistemological predilection for superstructural determinism are small defects that detract little from the immense empirical and theoretical value of this serious, scholarly work.

Presbyteries and Profits: Calvinism and the Development of Capitalism in Scotland 1560-1707. By Gordon Marshall. Oxford: Clarendon Press, 1980. Pp. x+406. \$54.00.

Jere Cohen

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Max Weber's critics have claimed that the case of Scotland contradicts his hypothesized link between Calvinism and capitalism since Scotland's dominant Presbyterian faith is Calvinist yet the country long remained economically backward. In *Presbyteries and Profits* Gordon Marshall argues that, to the contrary, Scottish backwardness was due to a host of inhibiting economic factors and occurred despite the presence of the "spirit of capitalism." He sets out to show empirically that the Scottish case actually vindicates Weber, since (a) Weber never claimed that the spirit of capitalism could produce capitalism when other necessary factors were absent, and (b) Scottish Presbyterianism produced a Scottish spirit of capitalism just as Weber would have expected.

Marshall does show that 17th-century pastoral writings stressed diligence in an occupational "calling" and ascetic abstinence from drinking, gaming, and other pleasures, just as Weber had indicated. However, it is entirely possible that these doctrines operated without the support of the predestinarian salvation doctrine as a psychological sanction. The Presbyterian pastors cited by Marshall recommended as the prime means of assurance of salvation *not* intense worldly activity, as Weber had hypothesized, but "a true and assured faith" (p. 66), which could be tested introspectively: Marshall notes "a strong subjective element in confirming one's elect status" (p. 66). The doctrines of hard work in a calling and of testing one's salvation were preached separately from each other, but Marshall feels that Presbyterian believers connected the two through a type of chain reaction. In order to demonstrate his election, a person would have to prove that his faith was true; since "good works" were believed to flow from true faith, a person needed to live a godly life to confirm his faith; and since a godly life included hard work in a lawful calling, intense occupational effort was

needed to confirm his election. However, Marshall provides no evidence that 17th-century Presbyterians were making these connections.

In attempting to show the simultaneous presence of the neo-Calvinist ethic and rational capitalism in Scotland, Marshall demonstrates that a cloth mill and a coal mine used such rational capitalistic practices as planning, labor discipline, work by the clock and calendar, double-entry book-keeping, inventories, careful accounting and record keeping, balancing the books, continuous enterprise, and gearing production to market demand. One finds in these firms the spirit of capitalism in the sense of a nontraditional, rational, calculating quest for ever-renewed profit (although, Marshall says, not in Weber's second sense of "a duty of the individual toward the increase of his capital" [p. 19]). It is unfortunate that detailed data are available for only two firms, since we cannot know to what extent the spirit of capitalism was established in 17th-century Scotland.

Perhaps the book's best original contribution is its demonstration of a connection of hard work and asceticism to rationality within the business firm. Without steady work and asceticism, productive output and labor costs cannot be calculated; idleness and intemperance make behavior within the firm unpredictable. Many of the record-keeping and accounting practices of the Scottish firms were aimed at making the output and costs of labor in the productive process more controllable and calculable. One Calvinist mine owner, in attempting to impose a policy of inner-worldly asceticism on his labor force, was led to systematize work procedures and his record keeping. This example shows more clearly than ever before "the elective affinity between the Calvinist ethic and the spirit of capitalism" (p. 247) with regard to business practices.

As regards the justification of profits, however, the link between Calvinism and the spirit of capitalism remains tenuous. Although Calvinism's twin injunctions, to work and not to enjoy the fruits of one's labor, led inadvertently to savings, profits, and reinvestments (p. 72), it is difficult to see how this idea complex developed into a justification of profit as an end in itself. Seventeenth-century Presbyterian pastors like Abernethy and Struther (pp. 82-85) did not condone profit as an end in itself. The possession of riches "onlie to have them" was avarice (p. 83) and "he that loveth riches without the fruit thereof" was "covetous" (p. 82). Gain was good only if put to good use. Neither Weber nor Marshall explains how this ethic led to its near opposite, the "entirely secular" justification of enterprise in terms of profit as an end (pp. 205-7).

To sum up, the neo-Calvinist doctrine of worldly asceticism was preached in 17th-century Scotland and encouraged the spirit of capitalism in one or two firms. Beyond that data are lacking, and we cannot tell to what extent Scottish Calvinism led to the spirit of capitalism. Therefore it remains entirely possible that the paucity of capitalist expansion in Presbyterian Scotland is attributable, in part, to a shortage of the "capitalist spirit." The data, while suggestive, are not extensive enough to nail down the book's main point.

The Shock of the New. By Robert Hughes. New York: Alfred A. Knopf, 1981. Pp. 423. \$29.95.

Vera L. Zolberg

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Periodically, sociologists are called upon to look more carefully at the content and nature of art works and to recast their research designs to integrate these works into societal contexts instead of focusing on analyses of organizational structure of cultural institutions, artistic professions and careers, elite versus democratic trends, art world networks and circles, political uses of the arts, and other recent research strategies.

Without attempting to evaluate the correctness of this alleged lacuna, or suggesting that, in the specialization which characterizes academic disciplines today, there may be some point in leaving the analysis of art works to those who know the most about them, we must acknowledge the general accuracy of the observation that relatively few sociologists have undertaken research with the degree of integration of content and sociological analysis that characterized some of the writings of Theodor Adorno, Max Weber, Georg Simmel, or H. D. Duncan. In any case, since social scientists have no monopoly on the subject of the arts (or on any other, for that matter), we should take seriously the works of specialists from other fields—critics, art historians, poets—who may deal with these topics from the standpoint of their own disciplines. While these specialists may treat the social side in a less systematic or obtrusive fashion than social scientists might, their vision, along with their expertise, may provide insights and inspire hypotheses which are not immediately evident because of our own scholarly myopia.

Thus John Berger's *Ways of Seeing* (New York: Penguin Books, 1980 [originally published in 1972]), based on a BBC television series which presents a simple (sometimes simplistic) but revelatory political reading of art works, has been joined recently by *The Shock of the New* of Robert Hughes, also based on a BBC television series, and shown on American Public Television in 1980–81. While Berger's work focused on art styles, particularly in portraiture, as subtle representations of ideology and used selected reproductions of paintings or photographs either as illustrations or, without words, as visual essays, Hughes attempts a sweeping explanation of the whole variety of avant-garde movements of the 19th and 20th centuries. His aim is to show the emergence of these movements as reactions against the academic rigidity which rejected "the millenarian optimism that surrounded the high machine age." He traces their increasing authority as convincing expressions of alternative ways of seeing in response to the rise of new technologies such as photography and motion pictures, as well as to new scientific outlooks, such as relativity and psychoanalysis. Historically this is followed by the despair and recognition of the absurdity of life and art that results from World War I, and a turning inward to psy-

chological states. Gradually the movements gained acceptance in the "institutionalized culture of late modernism" (p. 6), yet failed to achieve the political or social goals that some of their creators thought art would accomplish. Authoritarian regimes persecuted them, or the capitalism of liberal regimes deprived them of the shock value and impact which they had been intended to produce by absorbing them into the market (p. 383). As he succinctly puts it in another context:

Mass media took away the political speech of art. . . . It seems obvious, looking back, that the artists of Weimar Germany and Leninist Russia lived in a much more attenuated landscape of media than ours, and their reward was that they could still believe, in good faith and without bombast, that art could morally influence the world. Today, the idea has largely been dismissed, as it must be in a mass media society where art's principal social role is to be investment capital, or, in the simplest way, bullion. We still have political art, but we have no *effective* political art. An artist must be famous to be heard, but as he acquires fame, so his work accumulates "value" and becomes, ipso facto, harmless. As far as today's politics is concerned, most art aspires to the condition of Muzak. It provides the background hum for power. [P. 111]

Robert Hughes is by no means the first to have observed the impact of avant-garde movements. They have been designated as a "chronic condition of contemporary art" by Renato Poggioli and viewed (with some regret) as inevitable features of modern capitalism by Arnold Hauser. Hughes, however, is not only an analyst but one of the best-placed "tastemakers" of the contemporary art world, since he is the critic for *Time*. This affiliation may have advantages and disadvantages. On the one hand the book is replete with felicitous, if *Time*-ese, turns of phrase ("a great public artist like Gianlorenzo Bernini—that marble megaphone of seventeenth-century papal dogma . . ." [p. 367]). On the other hand it is well researched and contains many astute interpretations of a mass of material. Sociologists, however, may find some of his pronouncements too glib. For example, in arguing against the myth that vanguard artists are at odds with middle-class society, he agrees with Hilton Kramer, the influential *New York Times* critic, who claims that this is not a class but, rather, a generational phenomenon. In other words, the public for vanguard artists, Hughes asserts, has been composed since the mid-19th century of the children of the established middle classes. Put in this way, however, the generalization ignores what seems to have characterized these publics—that they were not primarily the scions of established groups, but of groups marginal to society—Jews, parvenus, foreigners. That they may also be younger is possible as well, but hypotheses on the subject need verification.

With the enormous audience that his handsomely produced book has reached, combined with the television audience in search of a legitimate intellectual framework for understanding the apparent inconsistencies, turn-of-abouts, and contortions typical of the modern art world, Hughes's work should be of interest to those who play direct roles in galleries, dealerships,

auction houses, museums, studios, or art schools. For these reasons, as well as the ones suggested above, sociologists of culture would profit from it as well, since it is likely to mold opinion or, at the least, have some influence on the outlooks of important gatekeepers.

Although Hughes provides few revelations, he has created an elegant synthesis of the diverse views known to critics, art historians, and other commentators. While he weighs their accuracy, he sometimes accepts their ideas too uncritically. His is an insider's account of modern art trends in which he attempts to establish plausible linkages between the works and the worlds in which their creators functioned. Without being exhaustive (the eight essays which compose the book are devoted mostly to painting and architecture), he touches upon and cogently illustrates what have come to be considered as—and what this book itself reinforces—the major trends in modern art.

On Becoming a Rock Musician. By H. Stith Bennett. Amherst: University of Massachusetts Press, 1980. Pp. xiv+258. \$15.00.

Samuel Gilmore

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For sociologists of art and music the problem is to map the organization of sight and sound onto the organization of society. The major limitation of this work has typically been the presentation of artistic content. The traditional sociological approach has been to utilize a "content analysis" which abstracts the meaning of the art work from the contexts in which it is produced and consumed in order to infer better its true significance for social organization. Unfortunately, this formalistic approach has not proved adequate in explicating the connections between the reification of forms of social production and the behavioral orientations of participants interacting with artistic objects. Past formal abstractions of content have simply not been relevant to the construction of these orientations.

Another approach to presenting artistic content is the one adopted by many musicologists and humanists who do not try to analyze abstractly the meaning of artistic objects, but rather try to describe concretely the processes used to produce and consume these objects. This is the same method used by symbolic interactionists to understand participants' "definitions of the situation" in any area of social life. The value of this approach in the sociology of art is that connections of artistic content to the collective construction of social organization may be developed along dimensions evidenced by participant activities, instead of being inferred through several levels of aesthetic abstraction. In *On Becoming a Rock Musician*, an extremely interesting piece of participant observation research by H. Stith Bennett, the connections between rock aesthetics and rock social organization are nicely developed from this type of empirical perspective.

Most of this work is based on two years of fieldwork Bennett did in Colorado playing with and interviewing members of local "copy bands" (i.e., rock groups that play other people's music). The rock aesthetic, by which Bennett means "the social appropriateness of music—its 'fit' with its audience" (p. 7), is a socially relative orientation embedded in the activity of playing rock music. His lack of concern with the philosophical valuation of rock aesthetics is entirely appropriate for this endeavor.

Following Howard S. Becker's work on the collective negotiation process which constructs and defines artistic activity socially, Bennett presents an exceptionally detailed account of precisely what this process entails for the emergent identity of young rock musicians. Chapter 1 looks at instrument acquisition, the development of preliminary skills, and finding someone with whom to play. Since rock music is characterized by: "processes of self-recruitment and learning without pedagogy" (p. 18), membership in the rock world is a function of mutual evaluation and shared participation in group formation and performance activities, rather than of certification and selection procedures established by some formally institutionalized organization. Bennett describes this complex interactional process in the continual formation and reformation of the primary social unit in rock culture, the group, which seeks levels of aesthetic compatibility among its members.

In chapter 2, Bennett elaborates the social ecology in which these activities and group constructions are enacted. The logistics of purchasing equipment, practicing, transporting the band, and most important, "getting gigs," all circumscribe a set of cultural and economic arrangements which mold the identity of rock groups along similar dimensions. For example, the allocation of performance opportunities is mediated by various types of social situations which demand different aesthetic responses from the group. The successful group (i.e., the one that survives economically) is one that is capable of recognizing the stylistic expectations of a particular audience and is flexible enough musically to satisfy a variety of needs. These groups get invited back or enhance their reputations sufficiently to generate future employment.

In the final two chapters, Bennett develops the core of his argument on the relationship between musical orientations and social organization. The context in which rock music participants construct aesthetic definitions of the situation reflects the resources of an aural environment he calls "The Music" (p. 113). These resources are the electronically produced and mass distributed recordings of American popular music which pervade the learning milieu of young rock musicians. Bennett looks at the use of these resources in practice sessions leading to the construction of "good performance" standards. He specifies this as follows, "For copy groups the problem of performance is to find ways to use the instruments and equipment of the live medium to approximate aesthetics originally produced through the recording medium" (p. 158). Indicatively, the highest compliment paid a copy group is, "You sounded just like the record" (p. 155). Bennett's point is that a recorded studio performance communicates a particular set

of aesthetic standards through which an imitative local band develops its live performance standards. The techniques of live sound control are derived predominantly from studio control exemplars even though the two situations do not share the same resources or technological capacities. Thus, a rock aesthetic is tied directly to the organization of copy-group socialization and performance activity.

Bennett should be given credit for accomplishing what he sets out to do. The relationship of copy-band aesthetics to the organization of their activities is quite clearly delineated. This research is successful from several other perspectives as well, including his exceptional attention to detail and well-grounded analytic constructs that lend themselves admirably to future elaboration. Bennett focuses on the social psychological processes of identity development while others, most notably Paul Hirsch and Richard Peterson, have addressed the macrosociological processes of institutional development. What is needed at this point is an integration of the two perspectives, which Bennett touches upon but does not fully resolve. This is a minor point, however, and Bennett's work is a substantial contribution to the sociology of art.

Juvenile Delinquency and Its Origins: An Integrated Theoretical Approach. By Richard E. Johnson. ASA Rose Monograph Series. Cambridge: Cambridge University Press, 1979. Pp. x+182. \$14.95 (cloth); \$4.95 (paper).

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University of Georgia

Juvenile Delinquency and Its Origins is designed to test the validity of strain, subcultural, and social control theories of delinquency. In testing these theories Richard Johnson develops a causal model that incorporates the major propositions of each of these theoretical traditions but is most heavily influenced by the social control perspective. He then tests the model, using cross-sectional data on the self-reported delinquent behavior of high school students.

The model that Johnson develops assumes that delinquency is situationally based, and "the crucial questions the model will address are as follows: (a) What forces determine the probability that a given juvenile will even be in such a situation? And (b) what factors influence the probability that deviance rather than conformity will be the situational outcome?" (p. 41).

In his design of the causal model to answer these questions Johnson demonstrates a clear and firm understanding of the theories being tested and of the results of past empirical tests of these theories. Moreover, he has integrated these theories as well as can be expected, given their somewhat different assumptions about human behavior and the temporal limits imposed by causal models. The empirically testable model that Johnson

develops includes measures related to the following concepts: social class, parent-child ties, school failure, future-oriented strain, delinquent peers, values and norms, and perceptions of the risk of apprehension.

The results of Johnson's work suggest that the major contributors to delinquent behavior are, in order of their importance: delinquent associates, delinquent values, and poor success in, and low attachment to, school. The findings did not support the theoretical perspectives tested, in that Johnson did not find strong effects on delinquency from social class, future-oriented strain, or even attachment to parents. He concludes that "the best theory would appear to be a combination of a class-free subculture (i.e., social learning) perspective with a social bonding orientation" (p. 140). In other words, on the basis of Johnson's data, social learning and social control theories appear to offer the most complete and economical explanations of delinquent behavior.

Although Johnson's work is marked by sound theoretical and empirical craftsmanship, there are a number of critical questions that can be raised about it. The first concerns the measure of the dependent variable. For a variety of reasons Johnson refuses to employ official measures of delinquency to indicate the dependent variable. Part of his reluctance is due to his assumption that official measures allow only for the division of his population into delinquent and nondelinquent statuses, but this is a problem only if one ignores the frequency of arrests and seriousness of the charges. Part of his reluctance is based on the potentially biased nature of police records, but recent research has shown that the biases are often more apparent than real. Whatever his reasons, he is left with a measure of delinquent behavior that is based on self-reported responses to eight items, the most serious of which is "attacking someone with a weapon other than your fists, willing to injure the person seriously if it came to that." One wonders if the results of his test, and the relative influence of the independent variables, would be replicated if the measure of delinquent behavior was more comprehensive with respect to the types and severity of offenses.

Similar problems surround the measures of some of the independent variables that were included in the model. For example, susceptibility to peer influence was measured by one item, social class was measured by data reported by high school sophomores concerning their perceptions of parental income and welfare receipt, and perceived risk of apprehension for delinquency was measured by two items. One wonders if the finding that these were the three least important concepts in accounting for variation in delinquency was due to their measurement or to their actual unimportance.

Other problems can also be noted. For example, only 7.9% of the sample were categorized as being members of the underclass and Johnson indicates that the sample underrepresented lower-class subjects and members of minority groups. Yet, despite this underrepresentation, Johnson is still willing to reach the conclusion that a "class-free" theory of delinquency is the best, and he also relegates social class to the category "variables of little or no importance" in the explanation of delinquency.

Although problems of this nature can be found in Johnson's work they must be put in proper perspective. They are nagging problems that place reasonable limits on the generalizability of the findings, but they do not detract in major ways from the overall importance or contribution of the book. Johnson took on a very ambitious project in his test of these theories of delinquency and the results of his work will inform the field for some time to come.

Radical Criminology: The Coming Crises. Edited by James A. Inciardi. Beverly Hills, Calif.: Sage Publications, 1980. Pp. 320. \$18.95 (cloth); \$9.95 (paper).

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Radical Criminology: The Coming Crises is one of a large number of Sage anthologies in criminology and criminal justice, which range from a number of "near books," containing relatively low-level articles, many of which are only loosely related to the volume title, at one extreme, to some excellent collections of essays coherently tied to a central theme, on the other. Although this volume is flawed by a number of instances of misspelled names and fractured syntax, it does fall toward the latter end of the scale.

Four of the essays in this book were first published in a special issue of *Criminology* (February 1979). However, Richard Quinney's views on Marxist criminology that appeared in that issue are not included in this anthology. Instead, the four papers included and most of the other essays are devoted to commentary on radical Marxist criminology rather than on the exegesis of the major arguments of that brand of criminological thought. James A. Inciardi could have improved this volume by including a detailed statement setting out the parameters and details of the new "paradigm" that is the focus of its attention.

Of the four *Criminology* papers, Carl B. Klockars's provoked a large number of complaints from radical criminologists. My reading of Klockars's piece led me to conclude that, although a number of his criticisms are on the mark, his critique is flawed by observations that are closer to ad hominem attacks than to reasoned criticisms.

In addition to the introductory statement by Inciardi and the Klockars essay, this volume includes papers by Geoff Mungham, David Friedrichs, Ronald Huff, Austin Turk, Jackson Toby, Ronald Akers, Milton Mankoff, Elmer Johnson, Steven Spitzer, David Shichor, Franklin Williams III, William Pelfrey, Stephen Pfohl, Anne Pottieger, Edwin Schur, Laurin Wolan, Jr., and Harold Pepinsky.

There is insufficient space here to review all of these essays. However, most of them touch on one or another of the now-familiar criticisms of

radical or Marxist criminology, many of which have to do with what David Greenberg has termed the "one-dimensional" character of that line of argument. Radical criminology is scored by these critics for being theoretically unsophisticated and historically naive. It is described by Turk and others as empirically shallow, depending more on partisan arguments and proof by assertion, anecdote, association, and analogy than on tests of evidence. Radical Marxist criminology is said by many to distort the writings of Marx. Other criticisms allege that radical criminologists tend to romanticize criminals; thus, according to Toby, they are disseminating the "old baloney," that is, false or misguided sentimentality. Along the same line, a number of these commentators note that radical criminologists have little or nothing to say about tactics that might reduce the real harm caused by garden-variety lawbreakers who are often members of the same underclass that is being victimized. Finally, radicals are criticized for their faulty understanding of contemporary socialist societies and particularly for ignoring the repressive nature of those systems.

A number of things can be said about this collection of essays. For one, the range of opinions expressed in them is wide, from Toby's, which finds nothing new or useful in radical criminology, to several others that view radical criminology as posing serious challenges to mainstream criminology. Second, these essays make it abundantly clear that it is a mistake to speak of mainstream and radical criminology as distinct entities, or to offer assertions about *the* "new criminology." For example, Ronald Huff indicates that there are a number of versions of conflict theory concerning deviance and crime and, moreover, that the dividing line between mainstream, pluralistic conflict claims and those of radical criminologists is not a sharp one.

A third observation is that some of the contributions are more tangential to the main thrust of the volume than others. For example, the contributions by Pelfry and Pfohl have more to do with the teaching of criminology than with the substantive claims of criminological theory.

My own view parallels that of Ronald Akers and others who argue that it may be possible to develop a nonpartisan and empirical brand of criminology informed by radical and Marxist arguments. Moreover, there are some signs that criminological analysis may be moving away from what has been termed instrumental analysis, which has characterized much radical criminological theory to date. Instrumental analysis may give way to structural analysis, which views the interests of different classes, social power, social control systems, and so on as being linked together in much more subtle, complicated, and less easily identifiable ways. One recent indicator of this kind of richly textured analysis can be found in S. D. Stein's essay on the sociology of law in the *British Journal of Criminology* (April 1980). Quite possibly, the contours of criminological analysis will be changed considerably in the next decade or so, in ways that will make much of the commentary in this anthology obsolete.

The Business of Organized Crime: A Cosa Nostra Family. By Annelise Graebner Anderson. Stanford, Calif.: Hoover Institution Press, 1979. Pp. ix+179. \$10.95.

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California Polytechnic State University

To judge by the rivers of ink that have been spilled over the issue of organized crime, one would think it was one of America's most pressing social problems. The testimony of Jimmy "The Weasel" Fratianno, the latest "mafia snitch," won extended coverage on CBS's "60 Minutes," a cover story in *Newsweek*, and front-page newspaper articles. *The Business of Organized Crime* is a needed antidote to the media's proclivity to focus on the sensational at the cost of balance and perspective. Indeed, the description of the Cosa Nostra family Annelise Graebner Anderson studied would hardly be recognizable to one who had read only the mass media accounts.

Like many other studies of organized crime, Anderson's relies almost exclusively upon data from law enforcement agencies. As a former project manager of organized crime research for LEAA and a member of the Advisory Task Force on Organized Crime, Anderson was given access to a substantial body of data gathered by what is identified only as "a federal agency." These data included reports from informants and field agents and from surveillance operations, and they were supplemented by interviews with law enforcement officials but not with the criminals themselves. Although the study was published in 1979, it relies on data from a decade earlier, primarily from 1968 and 1969. Although the difficulties, and perhaps even dangers, of doing research in this area are obvious, one cannot help wishing that the author had had more direct access to her subjects or at least some ability to direct the type of information being gathered.

Most of Anderson's work concentrates upon the activities of a Cosa Nostra family she named the Bengueras, who operated in an unidentified major city. The picture that emerges from her account of this group stands in sharp contrast to the image of the Mafia presented in the media. The Bengueras family, according to Anderson, were a group of aging Italian-Americans who were settled into a lucrative, but not luxuriant, illegal business. At the time of this research there were 75 "made" members of the family (average age 60) who worked with a numerous group of close associates. Although their income was founded in prohibition bootlegging, its principal source was now numbers gambling and loan sharking. The Bengueras family had an organizational structure similar to the general model described by Donald Cressey in *Theft of the Nation* (New York: Harper & Row, 1969), but the economic activities of the members were quite independent of the family structure, and some lower ranking members actually had higher incomes than their superiors. Anderson sees the family structure as a quasi government regulating the behavior of members,

settling disputes, and funneling payoffs to the right officials. Not only did the family explicitly discourage violence, it prohibited participation in activities considered to be too risky, for example, narcotics, kidnapping, and counterfeiting. The critical question we are not able to answer from Anderson's data is how typical the rather benign Benguerria family is of other organized criminal groups, and the fact that the real name and location of the family are concealed makes it extremely difficult to arrive at an independent judgment.

Anderson devotes considerable space in this short book to an analysis of the Benguerria family's legitimate business interests, and it is here that the limitations of her data become most apparent. She makes an effort to test five hypotheses about the reasons the family made legitimate investments by analyzing the types of investments, but often her data are not up to the task. Her analysis could have been greatly clarified by direct interviews with just two or three willing informants. (The five hypotheses are, in descending order of empirical support: establishing a tax cover for illegal income, providing a front for illegal business, serving family members, diversification, and profit.)

The only chapter of the book that does not deal with organized crime is in one respect its most important, for it provides significant new evidence in the debate about the structure of the Cosa Nostra. On one side in the debate, Cressey has argued that the Cosa Nostra is a formal organization with a clearly defined hierarchical role structure, while Francis Ianni (*A Family Business* [New York: Russell Sage Foundation, 1972]), on the other, holds it to be a traditional social system based upon personal relationships and without formal roles that continue beyond the participation of given individuals. The first chapter of Anderson's book reproduces numerous passages from the transcripts of the FBI's electronic surveillance of the office of Samuel Rizzo de Cavalcante, the boss of a Cosa Nostra family. The transcripts clearly show that the high commission which directs the overall organization, and the de Cavalcante family, contain formal continuing organizational roles very much as described in Cressey's work.

Despite the limitations of her data, Anderson's work is clearly a most valuable contribution to the study of organized crime and one that must be read by anyone seriously interested in this field.

The Army and the Crowd in Mid-Georgian England. By Tony Hayter. Totowa, N.J.: Rowman & Littlefield, 1979. Pp. x+239. \$22.50.

Craig J. Calhoun
University of North Carolina at Chapel Hill

For a long time, the 18th century had the reputation of being a peaceful interlude in British history. It came between the dramatic and widely

known political and religious struggles of the 17th century and the social dislocations and insurrections that accompanied industrialization in the 19th century. During the 1960s this reputation began to change. The prevalence of food riots, resistance to conscription, and, in the later years, riots on behalf of church and king gained increasing attention from historians. In particular, the "new" social history, concerned to look at events "from below" and not always from the perspective of kings and generals, contained several arguments that the 18th-century mob was not only more frequently in action than had been thought, but was also more organized and self-conscious. The most influential of such arguments were offered by E. P. Thompson. Tony Hayter's Ph.D. thesis, here published in what appears to be a minimally changed form, represents an attempt to look once again from above at these insurrections newly discovered from below.

In *The Army and the Crowd in Mid-Georgian England*, Hayter pays little attention to Thompson's work; indeed he does not cite any works of Thompson and his associates from the middle and late 1970s on the 18th century, not even those specifically addressed to problems of the rule of law (e.g., E. P. Thompson, *Whigs and Hunters* [London: Allen Lane, 1975] or D. Hay, P. Linebaugh, and E. P. Thompson, eds., *Albion's Fatal Tree* [London: Allen Lane, 1975] to cite only the most obvious). This book is emphatically not a work of social history. Its author dubs it "a study in administrative history" (p. ix), which is a fair enough assessment. Most of Hayter's attention is focused on the activities and problems of heads of the British War Office, between the 1750s and 1780s, as they were compelled to deal with popular protest at home. He offers some comments on the matter of tactics employed by troops in actual contact with crowds and makes preliminary statements regarding the "law of riot," the role of magistrates, the nature of the home army, and the character of the 18th-century mob. The bulk of the book, though, is a competent but very narrow study of relations between the War Office and local magistrates and military commanders.

Hayter draws on a thorough reading of primary sources but ignores the secondary sources and theoretical arguments which might have made his book more interesting. He offers sympathy to incumbents of the anomalous and ill-defined position of secretary of war, especially to Viscount Barrington, but tells us too little for us to understand the politics and constitutional debates which trapped the secretaries, let alone the larger social issues which kept the military recurrently deployed, usually despite its own distaste, against the English populace. There are really only 160 or so pages of text in this book, the rest being occupied with lists of which Horse Grenadier Guards were in what parish when, and with a surprising number of blank pages for a book printed in our new age of austerity. In an earlier age, austerity might have dictated that a work of such narrow scope and such a descriptive nature would remain an unpublished thesis, to be consulted in libraries by specialists. It offers enough information to have made the specialists' efforts worthwhile.

The Crisis in Sociology: Problems of Sociological Epistemology. By Raymond Boudon. Translated by Howard H. Davis. New York: Columbia University Press, 1980. Pp. vii+285. \$25.00.

Harold J. Bershady
University of Pennsylvania

Raymond Boudon's book, *The Crisis in Sociology*, a collection of essays now brought out in an English translation, was originally published in 1971. Alvin W. Gouldner's *The Coming Crisis in Western Sociology* (New York: Basic Books) was originally published in 1970. However linked the two books may appear in title, they are in fact utterly independent. Neither man, apparently, existed for the other. It is instructive briefly to compare the two books as each author was espousing an idea of crisis quite antipathetic to the other, at approximately the same time.

Gouldner's book offered a vision of apocalypse, a denunciation of the faults of Western society through the artifice of criticizing the faults of its supposed direct reflection, Western sociology. Notwithstanding his numerous references to Continental scholars, Gouldner's concern was directed overwhelmingly to American sociology. He therefore read the fate of American society in the course of American sociology. In excoriating the present state of sociology as having failed in its earlier promise, in heralding a dimly perceived millennial future in which a sociology would emerge that would be true to man, Gouldner situated himself in the wilderness, a self-proclaimed exile, holding up a redemptive ideal. The exilic status of the author, the hortatory tone of the book, the moral terms of the analysis, the object to which the entire book points—a sociology fulfilled and therefore the West, America above all, fulfilled—all of these place Gouldner's book, despite its neo-Marxist accent, within a distinctly American tradition. It is the tradition of the jeremiad as Sacvan Bercovitch has illuminated it in *The American Jeremiad* (Madison: University of Wisconsin Press, 1978). Gouldner was speaking with more of an American voice than the title of his book alone would lead one to believe, more perhaps than he and many of his readers were fully aware. He was part of the American flesh, not merely of the sociology, that he condemned. This too is the meaning of the quotation from Nietzsche that he used as the book's epigraph: "Here are the priests; and although they are my enemies . . . my blood is related to theirs."

Boudon's book, by comparison, acknowledged a permanent state of latent crisis within sociology. To the extent that sociology is unsure of its own groundings, it is more susceptible to external forces. The crises within sociology that often result, Boudon held, are derivative, reflecting not only perturbations in society but weakness in epistemological foundation. Sociology as a science can indeed achieve autonomous status, and stand firm as a discipline in its own right, but this will require clarification of its epistemological underpinnings. However disputed various sociological ap-

proaches and methods may be, an effort must be made to find points of commonality among them. Only then will it be possible to forge the rational, positive bonds that will shore up the sociological enterprise. In the reasoned tone of the book, in the ideal put forth of an autonomous science, a *discipline*, in the inclusion, indeed the rapprochement, of every kind of sociology within the envisioned Republic of Sociology, breathes the spirit of Rousseau, Comte, and Durkheim. Boudon's is a modern voice of the French Enlightenment. And as befits an apostle of reason, his reach is universal. He says that not merely in the West and in the near future but in all of sociology there has been, and is, a crisis. I doubt that the book would have received much notice in the United States had it been translated shortly after it first appeared. Perhaps the decade of the 1980s will be more receptive to the voice of universal reason.

Boudon's book consists of a series of critical studies whose aim is to reduce a few of the more persistent methodological and conceptual confusions. These and many more studies like them are needed before a precise, yet rich enough language can be developed that will allow diverse sociologists to communicate. The book is therefore a preliminary effort, an attempt to clear some of the debris that has stood in the way of epistemological construction. Nine essays in the book are devoted to this effort. The tenth, concluding essay is a diagnosis of the French university system and its relation to the student uprisings that received worldwide attention in May and June of 1968. This is, to my taste, the most interesting essay in the book, revealing important shifts that have occurred in French higher education since the Second World War.

As French universities have expanded, the proportion of students from the lower-middle class has grown steadily over the proportion of students from bourgeois backgrounds. But universities in France, unlike their German and English counterparts, have resorted to a variety of ad hoc teaching arrangements to meet this influx of students. The result has been to leave the bourgeois students increasingly displaced, the lower-middle-class students increasingly disoriented. The many antagonisms produced are somewhat analogous to the antagonisms that divide contemporary sociology. The parties on either side speak virtually different languages.

Indeed, in the preceding essays Boudon discusses a variety of sociological topics similarly contested: functionalism, neo-Marxism, aggregate interaction analysis, formalization, psychological assumptions, the Popperian philosophy of science. Two of the essays, chapter 5 ("Lazarsfeld's Metasociology") and chapter 7 ("Theories, theory and Theory"), are the most substantial pieces, comprising over a third of the book's contents. In these two essays in particular, Boudon has taken pains to separate the philosophy of science pertinent, perhaps, to the natural sciences from the philosophy of science that would be pertinent to the social sciences. Much unwitting mischief has been produced by imposing physicalist doctrines upon the social sciences. By observing closely what social scientists actually do, a philosophy of science truer to the "project" of the social sciences can be

achieved. Not until then will something like a grammar of the discourse of the social sciences be derived.

Many of Boudon's strictures are sensible and deserve agreement. Yet there are several critical questions he touches on but treats in an essentially unsatisfactory way. He acknowledges, for example, that there are powerful bonds between a paradigm and the noncognitive values of its adherents. But surely until these bonds are revealed and analyzed, his appeal to reason will be heard only by those who love reason. He assumes that the earlier Diltheyan and Weberian conception of a *verstehen* sociology is moribund, that its irrational weaknesses have doomed it to practical extinction. But in treating *verstehen* merely as an antique method he has failed to appreciate the more formal rigors to which *verstehen* has been put in the work of Habermas, say, or of Geertz, Goffman, Garfinkel, and Cicourel—examples of which were available at the time that he wrote the essays.

Is it too much to suggest that Boudon's passions have deafened him to developments in sociology that are animated by passions foreign to his own? This is not a matter of ideology in any crude sense, but of implicit differences in national culture. Neither the malice of a Gouldner nor the goodwill of a Boudon alone can overcome the hold of national culture; for this purpose the most careful, searching comparisons are needed.

I believe that Boudon's initial premise is correct but consider the objectivist assumptions of many of his arguments too restrictive. He seems unaware of more recent epistemological work congenial to the social sciences quite differently pitched from his own, the work of G. E. M. Anscombe, Karl Apel, Hans Gadamer, Charles Taylor, Georg von Wright. It is nevertheless enjoyable and challenging to work through Boudon's more sustained analyses. Boudon's book is a serious effort. The unpretentious, moderate tone gives the reader breathing space, room to disagree in the spirit of dialogue—no small consideration in recommending the book to others.

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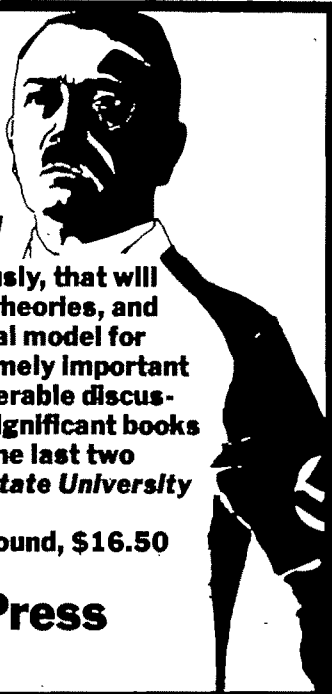
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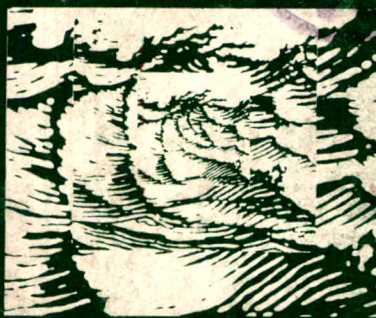
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IN THIS ISSUE

GLEN H. ELDER, JR., is professor of human development and sociology at Cornell University and director of the Life Studies Program, Cornell Institute for Social and Economic Research. As a Guggenheim Fellow, he is engaged in a comparative study of socioeconomic change and life-course development in two birth cohorts.

JEFFREY K. LIKER is assistant professor of industrial and operations engineering at the University of Michigan—Ann Arbor. He received his Ph.D. in sociology from the University of Massachusetts—Amherst in 1980, having previously earned a B.S. in industrial engineering from Northeastern University. He continues to collaborate with Glen Elder in the investigation of linkages among economic decline in the 1930s, family tensions, and psychosocial development. In addition, he is collaborating with a team of researchers at the Institute for Social Research on a project called "Psychological Factors in Economic Mobility of the Poor" and is researching the theoretical foundations of linear panel models of change with members of that group.

WILLIAM P. BRIDGES is associate professor of sociology at the University of Illinois at Chicago. His research interests lie in the areas of industrial sociology and social inequality. He is currently collaborating on an investigation of the social dynamics of a large metropolitan labor market.

STANLEY LIEBERSON is a member of the sociology faculty at the University of Arizona. He is currently working on a volume that seeks to improve the logic underlying social research procedures. In addition, he is doing the report on ethnic and racial groups for the 1980 census monograph series. Related to the paper appearing here is a model that Lieberman and Carter have developed for inferring the voluntary and involuntary causes of a group's segregation. It is scheduled to appear in the November 1982 issue of *Demography*.

DONNA K. CARTER is a Ph.D. candidate in sociology at the University of Arizona. She is currently studying eminence among American women and men in the 20th century.

GARY D. LAFREE is assistant professor of sociology at the University of New Mexico. His main interest is social reactions to crime. His current projects include studies of jury decision making, plea bargaining, and the criminal processing of Chicano defendants.

JOHN HAGAN is professor of sociology, law, and criminology at the University of Toronto. His work on the paper in this issue was done while he was at the University of Wisconsin—Madison. His recent work includes an article on the role of corporate victims in criminal court cases, forthcoming in *Social Forces*; a review essay on racial disparities in criminal sentencing, coauthored with Kristen Bumiller, forthcoming in a monograph; and a forthcoming article on the sentencing of white-collar offenders, coauthored with Ilene Nagel.

CELESTA ALBONETTI is a Ph.D. candidate at the University of Wisconsin—Madison. At present she is researching the determinants of control over bail decision making. In addition, her dissertation involves uncovering and specifying the effects of selectivity bias on the determinants of decision making at the various stages of criminal case processing.

MICHAEL HARRISON is senior lecturer in sociology at Bar-Ilan University in Ramat-Gan, Israel, and adjunct professor of organization studies—human resources management at the School of Management, Boston College. His research interests include professionals in complex organizations and the processes of organizational change and adaptation. He is currently completing a book entitled *Analyzing Organizations: A Diagnostic Approach*, which is a guide to diagnostic and field studies.

BERNARD LAZERWITZ is professor of sociology at Bar-Ilan University in Israel and director of its Institute for Community Studies. At present he is studying the urban renewal process in Israel with a primary focus on the degree to which working-class neighborhoods can organize themselves. In addition, he is directing research on the Israeli condominium system and is interested in contrasting major institutions in majority and minority contexts.

HANS ZEISEL is professor of law and sociology emeritus and an associate of the Center for Studies in Criminal Justice at the University of Chicago. Together with his late colleague at the Law School, Harry Kalven, Jr., he pioneered the use of social science research in the law.

PETER H. ROSSI is professor of sociology and director of the Social and Demographic Research Institute at the University of Massachusetts—Amherst, and coeditor of *Social Science Research*. He has been on the faculties of Harvard University, Johns Hopkins University, and the University of Chicago, where he also served as director of the National Opinion Research Center. His research has been concerned largely with the application of social research methods to social issues, and he is currently engaged in research on natural disasters and criminal justice. In 1979–80 Professor Rossi was president of the American Sociological Association, and in 1981 he received the Evaluation Research Society's Myrdal Award for contributions to evaluation research methods.

RICHARD A. BERK is professor of sociology at the University of California, Santa Barbara. His main interests lie in applied social science research, especially the interface between sociology and economics. His recent books include *Labor and Leisure at Home: Content and Organization of the Household Day* (with Sarah Fenstermaker Berk), *Money, Work, and Crime: Experimental Evidence* (with Peter H. Rossi and Kenneth J. Lenihan), and *Water Shortage: Some Lessons in Conservation from the Great California Drought, 1976–1977* (with Thomas F. Cooley, C. J. LaCivita, and Katharine Sredl).

KENNETH J. LENIHAN is associate professor at John Jay College of Criminal Justice, City University of New York. His current interests are the causes of crime and the evaluation of efforts to reduce it.

MICHAEL HOUT is associate professor of sociology at the University of Arizona. He is currently working on the demography of Hispanics in the Southwest and continuing the work on husbands' and wives' occupations reported in this issue. He has recently completed an analysis of the importance of autonomy and training for social mobility.

Information for Contributors

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Hard Times in Women's Lives: Historical Influences across Forty Years¹

Glen H. Elder, Jr.
Cornell University

Jeffrey K. Liker
University of Michigan

Economic change represents a major theoretical source of changing health states in populations and among individuals. The most impressive empirical documentation of this link at the aggregate level has come from time-series analyses. Studies at the individual level have begun to specify the causal elements in such relations, with emphasis on the variable health outcomes of economic deprivation. Specifying the duration of effect, however, requires longitudinal data. Such studies are still rare in the field and most are too limited in time span to test duration hypotheses. Using one of the oldest known archives of longitudinal data, this study examines the consequences of the Great Depression experiences of middle- and working-class women for their well-being in old age, 40 years later. Adaptations to human and material loss in the 1930s varied widely by social class, reflecting the greater severity of economic stress and the greater resource disadvantage of working-class families. With health measurements before the Depression and some 40 years later, the results show consistently adverse effects of economic loss for working-class women and benign outcomes among women of higher status. These long-term implications of decremental change point to the need for greater understanding of the life course in studies of the interplay of social history and life history.

A recurring theme in the human drama of rapid change centers on the fragmentation of lives and their internal discontinuities in a transformed environment. When such changes occur midway in life, initial purposes and directions lose meaning for the anticipated future. The ends of life may

¹ This study is based on a program of research on social change in the family and life course. Support from the National Institute of Mental Health (grant MH-34172, Glen H. Elder, Jr., principal investigator) is gratefully acknowledged. We are indebted to the Institute of Human Development (University of California, Berkeley) for permission to use archival data from the Berkeley Guidance Study. In the course of this research, Paul Allison, Paul Baltes, and John Nesselroade provided much-appreciated counsel on matters of design and analysis. Editorial recommendations by anonymous reviewers were most helpful. Requests for reprints should be sent to Glen H. Elder, Jr., Social Change Project, NG-22, MVR, Cornell University, Ithaca, New York 14853.

become obscure. Migration from rural to urban areas is one potential source of disjuncture in the life course; economic cycles provide another. Americans who grew up in Depression scarcity soon faced the problems and temptations of prosperity. Now their postwar offspring must cope with a period of retrenchment that calls for the scarcity virtues of thrift and conservation. In various ways, historical change thrusts people into new situations that can challenge the means, pathways, and ends of accustomed life.

An alternative or supplementary account of social change in life experience stresses the preparatory influence of certain historical transitions for subsequent life adaptations. This alternative account can be illustrated by the military and home-front demands of wartime America. Military service in World War II and the Korean War opened up developmental experiences and educational opportunities that improved career prospects for some veterans. Wartime employment opportunities also enabled some women to acquire skills and confidence by supporting themselves. Most occasions of social change defy a simple account of life effects by giving rise to contradictory or varied consequences, depending on the individual's exposure to changes and adaptations in the new situation.

This research explores the long-term consequences of one type of social change for the life experiences of women who lived through that change; we trace the influences of the economic collapse of the 1930s on the life-course development of a small number of women who were born at the turn of the century (from about 1890 to 1910). A common theme running through the Depression years and the later years of life for these women is the prevalence of loss. This study investigates the hypothesis that their degree of well-being in old age is partly a function of how they dealt with the problems of human and material loss during the 1930s, some 40 years earlier. Adaptations to loss are contingent on both the severity of the deprivation and the resources brought to the situation. From both perspectives, women in the middle class appear to have clear advantages over women in lower-status families. Indeed, few relationships in social science have been more widely documented than the inverse association between impaired health and socioeconomic position (Langner and Michael 1963; Kohn 1972, 1977; Srole 1978). Among plausible accounts of this outcome, one theme stands out: *health is a product of the interaction between stressor and resources.*

Women in this study started their families during the prosperous 1920s and experienced the bust and boom of the 1930s and 1940s as wives, mothers, and often earners. All are members of the well-known Berkeley Study (Macfarlane 1938), a longitudinal investigation of normal development in a sample of 211 middle- and working-class children and their families. The children were systematically selected from a list of Berkeley births in

1928-29. Approximately 81 of their mothers were interviewed during the 1930s and then again in 1969-70 (mean age = 70). As might be expected from class differences in mortality and social participation, high-status and well-educated women were more likely than other women to be contacted in old age (Maas and Kuypers 1974). These differences remain even with the deceased (about half of those lost in sample attrition) excluded from the comparison. Even so, the 1969-70 participants resemble other women of the original sample with regard to the incidence and severity of economic loss during the 1930s.

We view loss and adaptation as a conceptual bridge between hard times during early adulthood and later life some 40 years after the stock market crash. Losses after midlife generally represent a more expectable, culturally patterned sequence of experiences than the seemingly arbitrary deprivations of the 1930s. Nonetheless, the experience of having coped with such hardships in early adulthood is a potential resource among aging women who face social, material, and physical losses. On the other hand, a prolonged sequence of Depression misfortunes might have precisely the opposite effect through diminished inner resources and greater health risk when the inevitable losses and separations of old age occur. For middle-class families, the Great Depression often created a temporary economic setback lasting several years, while in the working class adults were more likely to experience the economic losses of the 1930s as more of the same economic misfortunes that already characterized their lives (Elder 1974). We begin the study with an examination of class differences in the social and economic resources Berkeley women brought to the 1930s and then employ measurement and causal models (Joreskog and Sorbom 1979) to assess their psychological health in old age. Some preliminary issues are worth noting as background to the research.

A life course perspective on aging assumes that individuals assess and react to new situations in the light of their personal biographies. The ups and downs of a lifetime furnish lessons, liabilities, and resources that influence the ways in which men and women age and meet the realities of later life (Butler 1975). The wisdom to make sound choices stemming from life experiences, positive and negative, may ensure a legacy of good health, adaptive skills, and material security in old age.

The course of individual development prompts a number of questions. What are the life course antecedents of successful aging (Neugarten 1970)? How are life events distributed across the life span (Pearlin and Johnson 1977)? Why do similar misfortunes during the early years appear to lead one person to become bitter (Bowlby 1980) and another grateful? How can we account for the large number of lower-status people who manage adversity so well in their lives (Mechanic 1972)? How do people in crisis or deprivation conditions manage without longstanding injury or impairments to

self or others? Can we account for this difference between successful copers and the people who become ill or disabled?

For American women born around 1900, answers to these questions involve recognition of differential exposure to historical change, such as the economic collapse of the 1930s, as well as the differential resources families brought to the situation. Some families were exposed to severe economic hardship; others managed to avoid misfortune altogether. We shall use economic variation as a starting point in assessing the long-term effects of the Depression experience among the Berkeley women. The strategy entails systematic comparisons of the psychological health and coping resources of women who suffered economic hardships in the 1930s with those of women who managed to avoid such deprivations.

PROBLEMS OF LOSS AND ADAPTATION: THE LATER YEARS AND PRIOR HARD TIMES

Loss is a common human experience during the later years of life, from the departure of children and the death of parents to disabling illness and the death of a spouse. These and other life changes often entail an undesired change of concept of self and other. Learning to handle such losses thus assumes primary significance as a developmental task. Successful aging may be linked to early experience which tests and develops a person's mettle in coping with the disruption of losses, with the problem of lost attachments and purposes. In this sense, the untested may be ill-prepared. For other survivors in old age, early events of loss and separation that once seemed uncontrollable and meaningless may increase emotional vulnerability to even minor constraints and setbacks. Brown and Harris (1978) identify loss of mother before the age of 11 as just such an event in the lives of women; the nature and timing of this loss impaired their feelings of mastery and self-esteem and prevented the effective working through of grief.

Depression Losses and Personal Control

The Great Depression has unique relevance for women's lives in old age through the common and profound experience of loss. Decremental events are common to Depression life and to life situations in old age, and such changes are generally beyond personal control. Family experience in the thirties often entailed severe income reductions and prolonged unemployment; the irretrievable loss of a family home and furnishings; the collapse of educational plans for a child; the trauma of financial dependence and shared living quarters; the disability, death, or separation of a spouse (Cavan and Ranck 1938; Elder 1974). A way of life had come to an end for many (Weigert and Hastings 1977). As the Depression years did, aging

generally entails increased exposure to loss, for example, the departure of the youngest child from home, widowhood, major illnesses, and retirement of self and spouse (Palmore 1974). Hence, Depression hardship offered a potential form of apprenticeship for women in learning to cope with the inevitable losses of old age.

The central idea relating Depression experiences to the degree of well-being in old age assumes that successful coping with loss builds confidence and resources for dealing with such problems in future situations (see Bandura 1977). For many women, Depression hardship meant the opening up of opportunities and challenges they had never before experienced. The daily round of Depression life for women expanded as the world of their unemployed or deprived husbands shrank. When men lost jobs, income, and a sense of meaning in life, their wives became central figures in an undermanned field of obligations and activities, from establishing and managing a home, to acting as the caretaker of a relative and the child rearer, to being the breadwinner and head of household (Scharf 1980). Survival needs pushed the managerial skill of women to more demanding levels. Many women were drawn into the financial transactions of borrowing money from banks and relatives. They also managed to get along on less by reducing purchases to the bare minimum, and they entered the labor force despite adverse public opinion about women employees.

A number of events underscored the value of a woman's independence: the need to share a cramped household with relatives, the risk of depending on the economic achievements of a husband, the domestic burdens that fell upon women as their husbands withdrew into themselves. Whatever the lessons drawn from such experiences, a large percentage of the Berkeley women assumed more responsibility for family income by entering the labor market. Nearly half of the women in hard-pressed families worked at some point during the 1930s, and a sizable proportion continued to hold a job in the 1940s. Though most jobs in the thirties were part-time, they made women more independent and provided skills that bear on successful aging. Recovery from the death of a spouse is especially problematic among older women who are highly dependent on their husband and do not have a work life of their own (Weiss and Parkes 1978). Through male default and pressure to work, Depression realities may have shaped a model of self-reliant womanhood that is uniquely suited to the living requirements of widowhood.

To be sure, such developmental benefits of hardship cannot be realized by those who fail to take control of their life situations and adapt in healthful ways. For example, men in deprived households in the 1930s often responded to economic loss with feelings of personal inadequacy, despair, and withdrawal from family and community life (Elder 1974, chap. 3). Although economic misfortune was not the exclusive fault of men, they

consistently focused the blame on themselves. Their own self-attribution, as well as societal expectations of men as economic providers, limited their options. Unlike the maladaptive responses of men, women's expectations and adaptive options opened up ways of coping that could prove healthful in the later years of life.

These issues suggest four conditions that bear upon the long-term effects of economic deprivation: (1) the degree or severity of the loss; (2) adaptive resources and options brought to this new situation (education, problem-solving skills, sense of efficacy); (3) definitions and assessments of the situation (causal attribution, cognitive appraisal, etc.); and (4) action or responses, such as job seeking or taking in boarders. The first two conditions influence definitions and responses (fig. 1), which can produce such varying outcomes as enhanced efficacy and sense of control, on the one hand, and social withdrawal and disorganization, on the other. These outcomes, in turn, affect the likelihood of subsequent diffi-

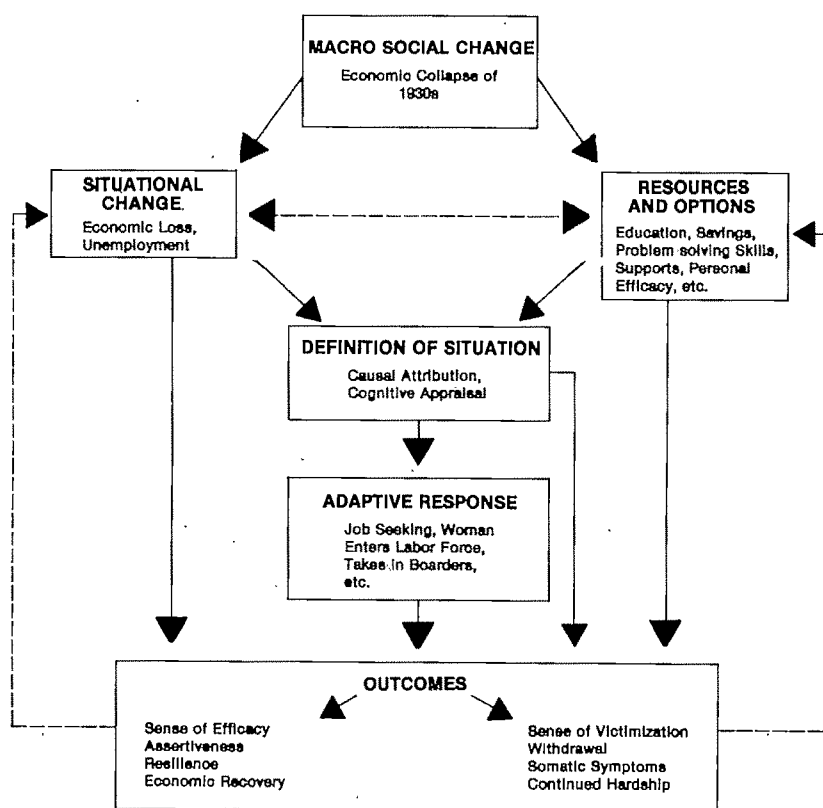


FIG. 1.—Theoretical model relating macro social change to individual adaptations and outcomes.

culties and the development of effective coping strategies, perpetuating cycles of advantage and disadvantage (Bandura 1977; Duncan and Morgan 1980; Peterson 1980).

As the Berkeley women entered the 1930s, class position tells us something important about their Depression experience (severity of loss, adaptive resources, modes of response) and later life in the 1960s. According to the Oakland study (Elder 1974, chap. 3), loss of status was especially painful to middle-class women, but they had more resources than lower-status women for dealing effectively with family hardship. Their educational, economic, and status advantages were expressed in stronger feelings of personal worth and more developed skills in problem solving. In contrast, working-class women were more vulnerable to the very real economic hardships that often seemed unmanageable. For them, family hardships reinforced feelings of inadequacy and helplessness. These issues are consistent with a "class interaction" hypothesis (see Kohn's [1972] interaction model): low socioeconomic status among women before the 1930s increased chances that hardship would impair their life prospects in old age (economic, health). Beneficial effects from Depression losses are found mainly among women who entered the 1930s as members of the middle class.

Pathways to Health and Illness in Old Age

Causal paths between widely separated events and states in the life course generally represent uncharted territory in studies of economic change and health. Little attention has been focused on the process by which early deprivation might persist through beliefs or personality or come to the surface at the end of life in feelings of persecution and inadequacy. The actual form of this association may vary from a diminishing relationship over time to a relatively stable outcome or a lagged effect in which the full impact is not observed until many years have passed.

Lagged effects in aggregate studies of economic change and health outcomes have stirred much controversy on temporal assumptions and by doing so have highlighted the utility of a life course perspective. The essential problem, as Brenner (1979, pp. 168-69) puts it, concerns "timing," the question of how long to observe the independent and dependent variables: "One may not see within a period of months, weeks, or even a couple of years what we would expect to see, given our most reasonable hypotheses, simply because the measured effect requires a longer period for observation." In some cases, the issue of time lag cannot be dealt with because of the constraints of a short-term panel (Kasl 1979). Brenner cites a two- to five-year lag as an appropriate assumption for aggregate time series, a premise that is not informed by knowledge of the life course and its continual interplay with historical change. From what we know about life-

span development (Baltes, Reese, and Lipsitt 1980), the lag should vary according to sex, career stage at the time of economic change, and the nature of the economic change. The full impact of early economic privation and insecurity may not be apparent until after retirement, when men or women enter situations that bear some resemblance to this early experience.

If deprivation has been severe, its cost in emotional and physical health is most likely to persist if deprivational conditions persist or return, as in old age; and if adaptive skills are impaired through ways of thinking (such as the misinterpretation of events [see Beck 1967]), feeling, and acting (Elder 1974, p. 241). The most obvious path for the negative effects of family deprivation on health lies in adverse economic situations in old age (Shanas 1962). Unemployment and heavy income losses during the 1930s forced unknown numbers of families to give up assets that provided security—a home of their own, other real estate, stocks and bonds, insurance policies. We do not know how prevalent such losses were among the Berkeley women or whether they represented more or less permanent reductions in their material status up to old age, about four decades after the stock market crash. But persistent strain seems most likely among members of the Depression working class. The better educated are more successful in building assets for security in old age than the less educated (Henretta and Campbell 1978); their net worth in later years is greater than expectations based on earnings, while the reverse pattern holds for the less educated.

Another plausible social linkage for the negative effects of economic deprivation entails a reduction in women's social support during the later years through fertility control in the 1930s or loss of spouse by means of an early death, separation, or divorce. Economic losses caught the Berkeley women in the midst of childbearing and may well have led to a change of plans regarding more children. Especially in the working class, the older women from deprived households may have fewer children to call upon than elderly women who lived through the decade in the privileged sanctuary of a nondeprived family.

Design of Analysis

The analytic design is structured by two general sources of life course variation: class position in 1929² as a proxy measure of the resources women brought to the 1930s (see Kuypers 1974; Kessler and Cleary 1980), and exposure to drastic income loss between 1929 and 1933. Not all women

² High-status or middle-class women in 1929 are defined by membership in strata I and II on the two-factor Hollingshead index (education and occupation) and in stratum III by marriage to a man with at least some college. Ordinarily, a woman must belong to all three strata to be defined as middle class. However, our use of a small sample called for a definition which subdivided stratum III by husband's education. Without this decision, we would not be able to investigate the life course of women from the lower socioeconomic strata.

experienced such loss. Indeed, some were spared misfortune altogether. Taking into account the sharp decline in the cost of living within the San Francisco Bay area (about 25% by 1933), the correlation between income and asset loss, and past measurement decisions in the study, we classified all families that lost at least 35% of their 1929 income as economically deprived. Less hard-pressed families were defined as *nondeprived*. During the early 1930s, slightly more than a third of the middle-class women in the original sample but more than half of the working-class women were members of deprived households.

The analysis is organized in three phases. We begin with women's resources when entering the early 1930s as young mothers. Do we find differences in social support and problem-solving skill between women from high- and low-status families or between the economically deprived and the nondeprived? The most general issue here is whether social class and economic loss produced different pathways for women and their life outcomes. The second part of the analysis puts the "class interaction" hypothesis to a direct test by comparing the effects of the Depression on the mental health of elderly women from the middle and working classes. The analytic technique, LISREL (see Joreskog and Sorbom 1978), enables a comparative assessment of these effects in the middle and working classes while taking measurement error into account. The estimated models include pre-Depression psychological states so that analyses examine change in relative functioning as an outcome of differential exposure to Depression hardship (Bohrnstedt 1969). Finally, we examine the long-term effects of economic loss on certain attitudes and behaviors that are related to an efficacious life-style for women in the later years.

In sample composition and design, this longitudinal study addresses a recurring causal issue in studies of economic change and health: the indeterminate causal picture (Kasl and Cobb 1979). Does ill health result from or lead to unemployment? Dooley and Catalano (1980, p. 452) conclude that "existing studies have failed to agree on the causal direction of the poverty-disorder association." Sophisticated causal models (Kohn 1977; Miller et al. 1979) indicate that relations between social structure and health involve both "social causation" and "social selection" processes. The long-term framework of this study and the particular change variable, based on loss of the husband's income, help to clarify the causal direction of influences that link Depression hardship to women's health in old age.

The Great Depression established something akin to a "natural experiment" by exposing families to relatively deprived and nondeprived experiences with minimal regard for their particular life history in the middle or working class. Our research design closely follows Gerald Klerman's (1979, p. 138) recommendation concerning the "strongest possible methodological design" for future research on social change and health: "to

take one or more factors presumed or demonstrated to be associated with higher risk, such as loss, bereavement, or the closing of a factory, and follow . . . a sample prospectively through time to observe the emergence of the presumed psychopathology" Though limited in size and composition, this longitudinal sample of Berkeley women represents the only known opportunity to trace the effect of the actual Depression experience of adults on their well-being and problems in the later years of life.

WOMEN'S RESOURCES FROM CHILDBEARING TO OLD AGE

The interaction hypothesis specifies class variations in the severity of economic loss during the Depression years and in the problem-solving resources women brought to this period of economic change. That the severity of Depression losses differed by class is well documented for a sample of families in Oakland (Elder 1974), and the trends seen there apply to the comparable sample of Berkeley families. First, working-class families were poorer before the Depression, and they lacked the savings and assets to absorb a large reduction of income. In such circumstances, the loss of even a small percentage of income can have a sizable impact. Second, income losses in the 1930s were more prolonged for working-class families, often lasting into World War II, while middle-class families typically returned to their pre-Depression incomes in a few years. Moreover, class variations in resources persisted through the postwar period into the later years; the best predictor of financial strain for the Berkeley women in 1969 is class position in 1929.³ Even working-class women who were spared economic misfortune in the 1930s experienced more severe economic privations in old age than middle-class women who lost heavily in the Depression.

Class variations in women's problem-solving resources in 1930-31 (before Depression losses for most of these families) are also evident. Eight indicators of problem-solving resources fall into three conceptual domains (fig. 2)—intellectual ability, emotional health, and physical health. Confirmatory factor analyses (not shown here) demonstrate that the interviewer ratings measure underlying factors that correspond to these concep-

³ Using a three-item measure of financial strain in old age, we estimated the independent effects of five conditions that bear upon economic welfare at this time of life. Financial strain represents the sum of three standardized interview items: (1) economic status—affluent, no worries or hardship; (2) home status—superior, all other; and (3) neighborhood status—superior, all other. Codes and β coefficients for the independent variables are: (1) retirement—0 = husband or wife works, 1 = retired ($\beta = .25$); (2) marital status—0 = unmarried, 1 = married ($\beta = -.14$); (3) economic deprivation—0 = non-deprived, 1 = deprived ($\beta = .10$); (4) social class in 1929—scores from 1 (low) to 5 ($\beta = -.51$); and (5) career stage—0 = birth before 1900, 1 = birth 1900 or later ($\beta = .15$). $R^2 = .40$.

tual distinctions.⁴ The LISREL models in figure 2 take measurement error into account and show the association between social class, as measured by the Hollingshead two-factor index of husband's occupation and education, and each of the three factors.

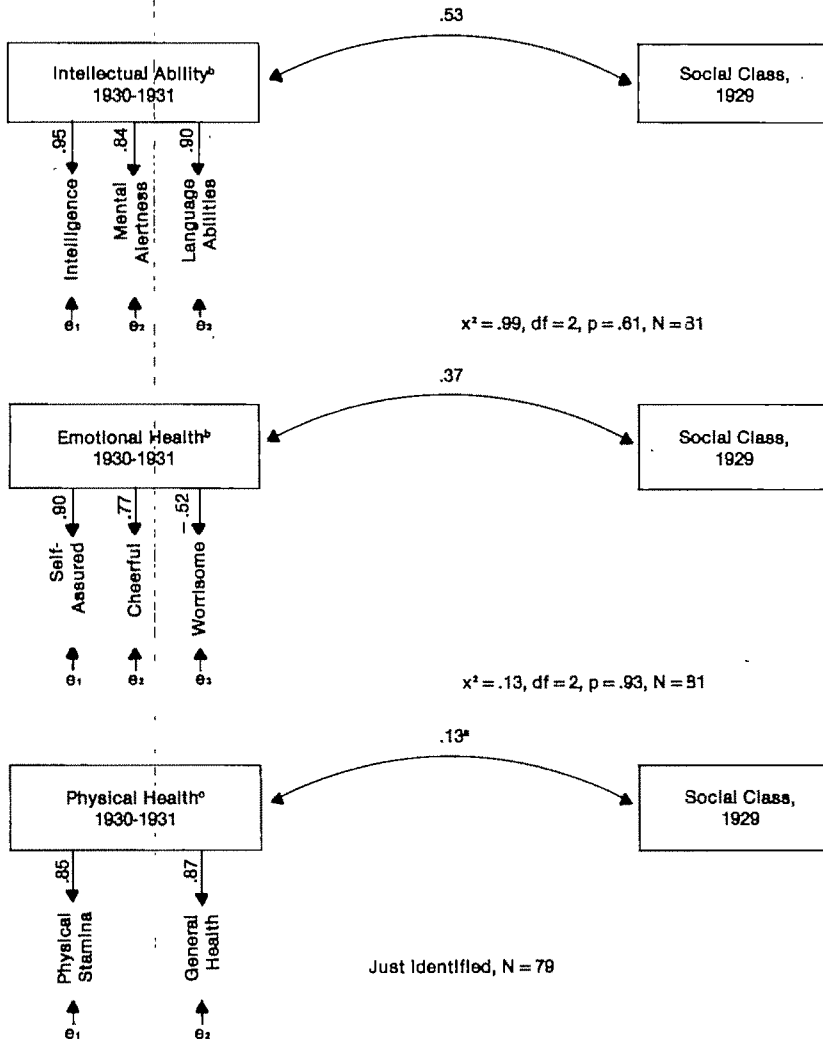


FIG. 2.—Class variations in women's intellectual abilities, emotional health, and physical health in 1930-31. *a*, Not significant at .05 level, two-tailed test. *b*, Ratings include seven points and represent an average of two clinical assessments. *c*, Ratings include five points and represent an average of two clinical assessments.

⁴ The three intellectual ability ratings are all reliable with standardized factor loadings ranging from .75 to .95. The emotional health loadings range from .49 to .91, and the

These correlations demonstrate that middle-class women rank well above women of lower status on qualities that could make a difference in the management and solution of Depression-related problems. Consistent with their higher education, higher-status women were judged brighter, more verbal, and more articulate by the interviewer ($r = .53$). In addition, social class is related to such emotional strengths as self-assurance, cheerfulness, and the lack of a worrisome outlook ($r = .37$), though practically unrelated to physical health ($r = .13$). In general, higher-status women had more in reserve cognitively and emotionally when they entered the 1930s, a decade that magnified many times over the life stresses that ordinarily pile up during the childbearing years. Hence, even before the Depression, working-class women were especially vulnerable to setbacks, barriers, and criticism, factors that soon became commonplace.

Apart from class position, which tells us something important about women's pre-Depression resources, there is no evidence that families hard hit by Depression losses differed in significant ways from families who were spared economic misfortune (Elder 1982). As noted earlier, the Great Depression appears to have produced a close approximation to a randomized experiment. Moreover, the objective life course of the Berkeley women shows little evidence of the Depression's imprint beyond the 1930s.⁵ There were no differences between deprived and nondeprived women on kin support in old age, and there is only weak evidence of enduring economic privations attributable to the Depression. Nonetheless, economic loss did shape the lives of these women in at least two ways.

First, women in the deprived middle-class group were much more likely than their nondeprived counterparts to go to work in the 1930s and to continue into the postwar years. Indeed, in the late 1930s (1936-39), 41% of the high-status women in deprived homes spent some time on a job, while only 10% of the nondeprived middle-class women did so. This difference was much less pronounced in the working class, where women in both the nondeprived and deprived groups commonly entered the labor market. In the late 1930s, 49% of the deprived and 43% of the nondeprived

physical health loadings are .60 and 1.13 for "physical stamina" and "general health," respectively. Only "mental alertness" bridged two conceptual domains with a modest link to emotional health (.32) and a strong link to the intellectual ability factor (.75). Four correlated errors were detected, three of these involving a physical health indicator. The goodness of fit with 81 cases was 21.7 with 11 degrees of freedom, $p = .02$. The poor fit and correlated errors were due to the physical health indicators which were available for only 53 cases (the Guidance sample, see Macfarlane [1938]). Since pairwise deletion was used, the N of 79 cases was overstated for the covariances involving physical health indicators. A model with these indicators removed fit at acceptable levels ($p < .05$).

⁵ A comprehensive comparison of the deprived and nondeprived Berkeley women on employment patterns, economic resources in old age, and social supports in old age can be found in Elder (1982).

working-class women spent time in the paid labor force. Hence, any developmental value of working is likely to be confined to middle-class women who generally had the luxury of staying home if their families were sheltered from Depression hardship.

Second, deprived middle-class women were much more likely than the nondeprived to be widowed in 1969. Among the deprived high-status women, only one-fourth of the 1929 marriages survived to 1969.⁶ This figure, well below the 71% survival rate for middle-class marriages in families that were spared major setbacks in the 1930s, may reflect the pathogenic effects of job and income loss on men (see Cohn 1978; Brenner 1973, p. 973). In any case, the high rate of widowhood among women from the deprived middle class is manifested in their relative disadvantage with regard to financial status (see fig. 6). Widows generally rank below married women in economic status.

Much of the literature on economic stress would lead to the expectation that working-class men who experienced prolonged hardship were subject to an exceptionally high probability of ill health and an abbreviated life span. Yet widows are not unusually common in households of the deprived working class, the group hardest hit by Depression losses. A number of factors could account for this disparity, including differences in attrition by deprivation status. As noted earlier, the participants in this study represent a biased sample of class origin relative to the 1930 sample; a disproportionate number of working-class women dropped out of the study between 1930 and old age. The working-class women who dropped out and their husbands may have been among the most damaged by hard times. Such biases have special relevance to the "interaction" hypothesis if indeed the deprived working-class women who were interviewed in old age displayed more adaptive skills than the drop-outs.⁷ From this vantage point, any long-term negative consequences of Depression hardship would be understated in the working class. Whatever the reason for the differential rates of widowhood, its greater prevalence among the deprived middle-class women may have strengthened their independence and need to work. For

⁶ Divorce and separation were equally likely for the deprived and nondeprived middle-class women (about 6%). However, 53% of the deprived group were widowed and did not remarry, compared with 16% of the nondeprived. It is interesting to note that 17% of the deprived women in this stratum remarried, compared with 6% of the nondeprived. Because they represent almost the same proportions of the total rate of divorce and widowhood for each group, these figures show that members of both groups were equally likely (or unlikely) to remarry.

⁷ Age variations by deprivation and class are not a factor in the high rate of widowhood among women from the deprived middle class. These women are not older than women in other subgroups; thus no meaningful difference in the age of husbands is implied. A check of the birth years of both wife and husband did not produce differences by class origin or economic loss.

example, women were more likely to be employed during the postwar era if their marriage ended because of divorce or death of spouse.⁸

Considering the available data on economic welfare and social networks in old age, there is little evidence of markedly different objective pathways from the Depression into the later years. Possibly, postwar affluence eliminated many of the economic disadvantages of the Depression (Elder and Rockwell 1978). Hence, any long-term effects of the Depression are likely to operate through psychological resources and adaptive capacities. Resources brought to the 1930s lead to the expectation that economic deprivation will have a negative effect on well-being in old age among the working class and a possibly benign effect among the middle class. Limited resources and acute survival pressures in the deprived working class increased the risk of helplessness among these women. Feelings of efficacy had a better chance of evolving through the adaptational and problem-solving efforts of women in the deprived middle class. Having managed on very little during the 1930s, these women could at least view the adversity of their later years with a belief that they had been there before and survived. Are such variations in health part of the Depression's legacy for women from the middle and working class?

EARLY HARDSHIP AND SUBSEQUENT WELL-BEING IN OLD AGE

The Berkeley women experienced the Great Depression of the 1930s when they were young, married adults in their early childbearing years. Our expectation that the Depression influenced their adaptive capacities assumes that personality is subject to change and development throughout the life span. Research on adult development (Miller et al. 1979; Brim and Kagan 1980) supports the view that intellectual functioning and emotional health continue to change in the adult years in response to changing environments and experiences.

To estimate the long-term effects of Depression hardship, we used a binary measure of economic loss or deprivation (nondeprived vs. deprived) to predict intellectual ability and health in old age;⁹ 1930-31 measures of

⁸ With 1945 SES, economic deprivation, and 1929 social class in the equation, single status during the postwar era predicted women's employment at some point during the period from 1945 to 1970. The β coefficient for marital instability among women from high-status families is .34—in the low-status group, the coefficient is .26.

⁹ There is some debate as to whether LISREL can handle binary independent variables since they do not satisfy the multinormal assumption. To test the sensitivity of the results to this binary assumption, we used a subgroup of the sample, 50 cases concerning whom detailed information existed that allowed us to construct a measure of the percentage of income lost in the 1930s. All LISREL analyses were repeated using regression analysis, first with the percentage-of-loss measure, second with the binary form of economic deprivation. The results were basically unchanged, supporting the interaction hypothesis, even though in all cases the binary measure yielded slightly larger coefficients.

ability and health were included in the model to take into account the stability (i.e., autodependence) of these characteristics as well as any pre-Depression correlations with economic loss. Since the interaction hypothesis identifies different effects of economic deprivation by social class, we established a test of the model in two status groups, high ("middle class") and low ("working class"). The truncated measure of social class in 1929 (Hollingshead index) served as a control for the residual effects of socioeconomic resources within strata. Covariance matrices were analyzed for all models, and all statistical comparisons and constraints across class strata are based on unstandardized coefficients. However, we present standardized coefficients to facilitate the comparison of coefficients within each stratum. The directional nature of the interaction hypothesis calls for one-tailed tests, but we also report the more conventional two-tailed tests (see figs. 3-4).

To account for stability and change in psychological functioning and health between the Great Depression and old age, a time span of nearly 40 years for the Berkeley women, we extended our measurement models of 1930-31 intellectual ability and emotional health to include equivalent interviewer ratings in 1969. The model for intellectual ability includes the three interviewer ratings in 1930-31 and again in 1969—general intelligence, mental alertness, and facility of language use (fig. 3). The model for emotional health also includes three ratings at two times—self-assured, worried, and cheerful (fig. 4).¹⁰ The lack of acceptable measures of physical health in old age ruled out a cross-time model on this dimension, although available self-reports in old age (Elder 1982) generated patterns similar to the results on emotional health.

Our dependence on interviewer ratings and the broad temporal scope of the study bring the need for reliable measurements and the degree of "real change" to the forefront as important issues (Nesselroade and Baltes 1979). Both issues can be addressed by developing LISREL models that make adjustments for measurement error in estimates of real change (Joreskog and Sorbom 1979).¹¹ A number of a priori decisions were made in con-

¹⁰ Measures of "intellectual ability" and "emotional health" were associated with measures of personal control and efficacy in the later years (average $r = .30$, not corrected for measurement error). The emotional health items were most strongly associated with indicators of "helplessness" (see fig. 7) in old age (correlations exceeded .40). The interviewer scales are called Tryon ratings of personal characteristics in the archive. The full list of characteristics exceeded 20, but a number of the scales were used only once or not at all or were used on only part of the sample. These six scales (three for each concept; see text) were used in the first effort to develop adequate measurement models of intellectual functioning and emotional health.

¹¹ Sample and subgroup sizes raised some questions at the outset as to the appropriateness of LISREL. The likelihood ratio χ^2 statistic is commonly assumed to be valid only when the sample size is large. However, recent monte carlo simulations (Geweke and Singleton 1980) suggest that the χ^2 in factor models is valid with subgroups as small as 30 cases.

structing these models for the three conceptual domains. We began by estimating models without correlated errors over time, a reasonable starting point since different interviewers and raters were used at the two measurement times, which were 40 years apart. Using procedures recommended by Sorbom (1979), we then built correlated errors into the model according to the observed first derivatives until the fit could not be improved by addi-

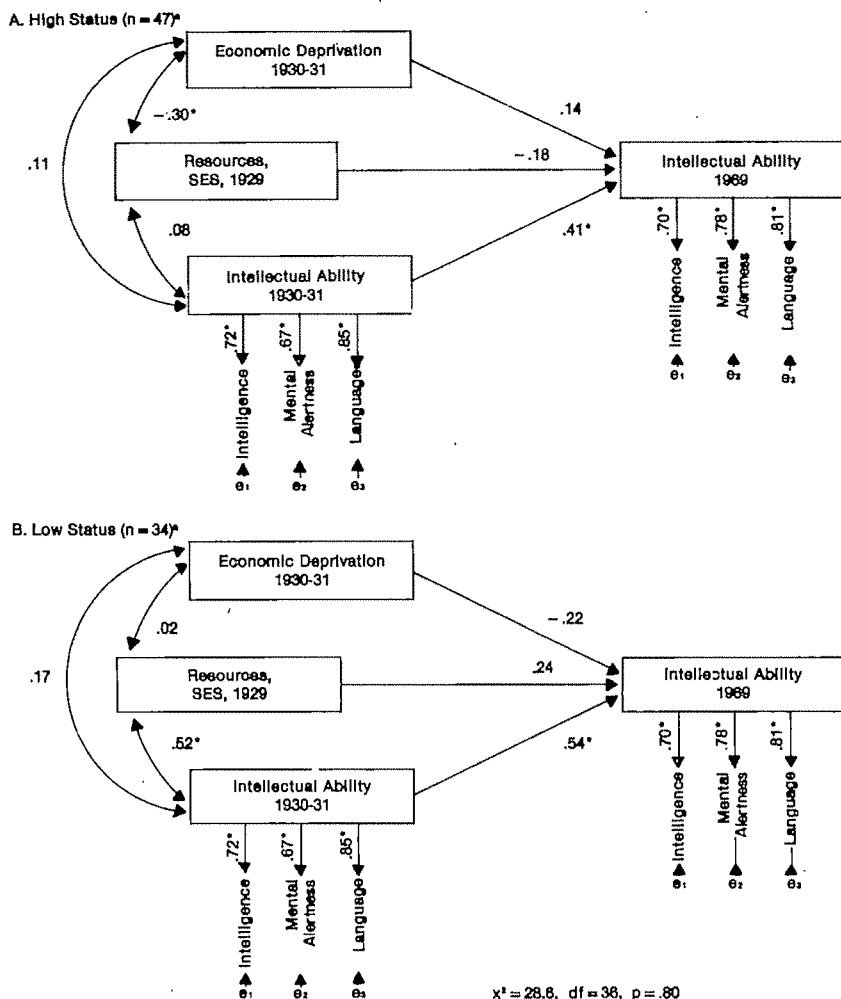


FIG. 3.—Effects of deprivation on intellectual ability in old age by 1929 class (standardized coefficients). * $p \leq .05$, two-tailed test; $^a p \leq .05$, one-tailed test (none in figure). Class differences in the effects of economic deprivation were not statistically reliable. a , Factor loadings were constrained to be equal across groups; all other parameters were free to vary by class.

tional adjustments. Note that of the six indicators in the cross-time models, only "cheerfulness" (fig. 4) has correlated error variance.

With the long time span and the very different life contexts (early adulthood and old age) of the two sets of measurements, we had no reason-

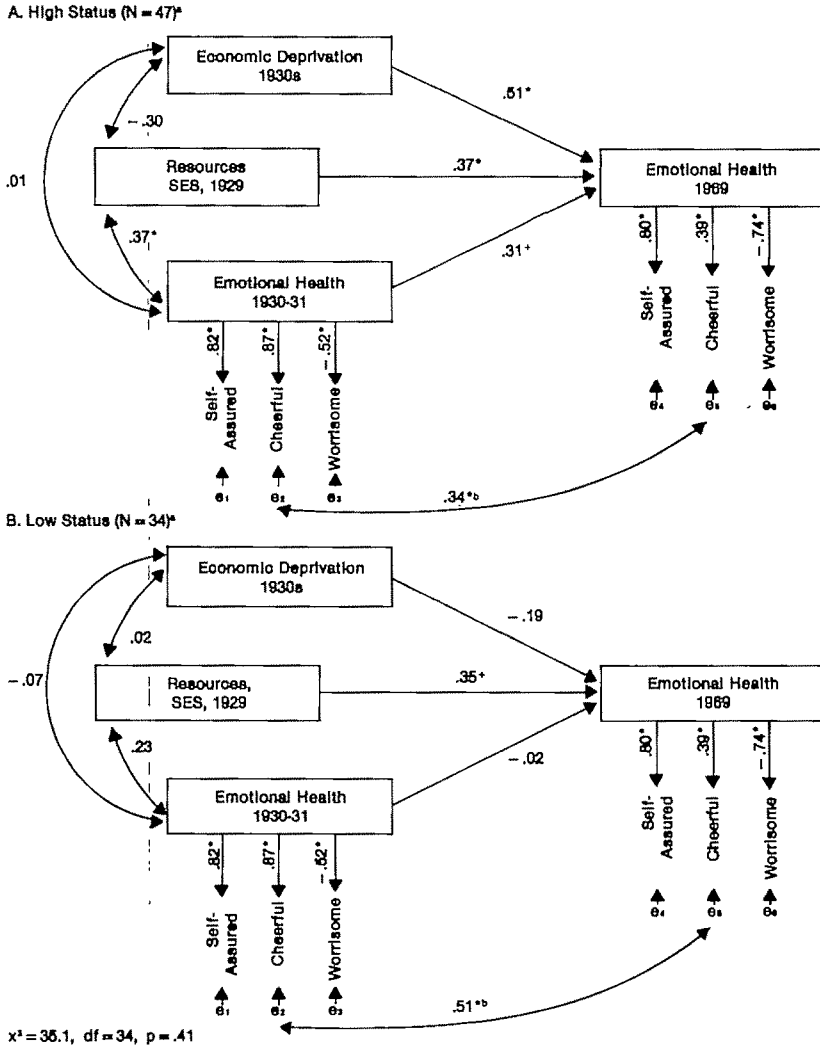


FIG. 4.—Effects of deprivation on emotional health in old age by 1929 class (standardized coefficients). * $p \leq .05$, two-tailed test; + $p \leq .05$, one-tailed test. The deprivation effects were significantly different across class strata on the basis of a t -test ($t = 2.4$), although estimating the model by fixing the deprivation coefficients to be equal across groups and then relaxing this constraint did not yield a significant χ^2 difference (2.2, 1 *df*). *a*, Factor loadings were constrained to be equal across groups, all other parameters were free to vary across groups. *b*, The correlated measurement errors in "cheerful" over time were detected by looking at the first-order derivatives (Sorbom 1979).

able basis for assuming that measure-construct relations were invariant across the two time points. Hence, we allowed the factor loadings to vary across time. The results suggest that "cheerfulness" is a more reliable indicator of emotional health during the early adult years of the Berkeley women (fig. 4) and that the absence of a worried outlook is a more reliable measure of emotional health in old age. Among other qualities, a worried state implies a lack of control over desired outcomes, a state especially common during the later years of life. Such variations in factor structure question the premise that we are dealing with the same construct of emotional health at the two time periods.¹² In contrast, the factor structure for intellectual ability appears to be invariant across the two time periods examined (fig. 3).

Finally, we used the simultaneous group analysis option in LISREL IV (Joreskog and Sorbom 1978) to constrain the factor loadings to be equal in both strata. No persuasive rationale came to mind for expecting different factor structures by class. In contrast, we did anticipate greater error in ratings of working-class women since the interviewers were all middle class. Hence, error variances, as well as all structural coefficients, were allowed to vary by class.¹³ The resulting models fit the data well ($\chi^2/df \leq 1$).

The cross-time coefficients correspond with the literature and with our expectations. Intellectual ability is more persistent across time than emotional health. Consistent with the literature on cognitive trajectories through life (Baltes and Baltes 1980), the mental acuity of women before the Depression tells us much about their intellectual status some 40 years later ($\beta = .41$ and $.54$ for high- and low-status women, $p < .05$). In contrast, emotional status during the childbearing years is not a reliable forecast of women's psychological health in old age ($\beta = .31$ and $-.02$, $p > .05$). In general, the overriding theme across these estimates is the prominence of life-span change. Even on intellectual ability, most of the variation during the later years of life for these women remains unaccounted for by intellectual variation during early adulthood.

During the thirties, middle- and working-class women differed in psychological resources that bear on effective coping, and some of these differences persisted through their later years. Personal losses in the 1930s thrust women into new, demanding situations that called on their inner resources. The changes placed greater emphasis on emotional resourcefulness than on intel-

¹² To test for structural invariance over time, we alternately constrained the factor loadings to be equal over time and allowed them to vary. The results showed that measure-construct relations remained constant over time for intellectual ability but varied significantly ($p < .001$) for emotional health. It is more accurate to say that we are looking at the effects of one emotional health factor on a different emotional health construct 40 years later.

¹³ However, we found no consistent evidence of greater error variance in the ratings of working-class women. The two strata are very similar in this regard.

lectual skills. Hence there is reason to expect the most enduring impact of Depression losses on emotional health or resourcefulness.

As a whole, the results support the interaction hypothesis (figs. 3-4). Economic loss diminished the emotional health of lower-status women in old age, as well as their intellectual capability. This legacy of Depression hard times is reversed among women who belonged to the middle class in the 1930s. The most vital, resourceful women from the middle class are those who were tested by the pressures of heavy income loss during the 1930s. As expected, the principal impact of the Depression experience appears in emotional well-being, not in cognitive skills, although the effect of deprivation on intellectual functioning appears substantial in the working class ($\beta = -.22, p > .05$; see fig. 3). The emotional costs of family hardship among working-class women may partly account for a mental decline over some 40 years; loss of self-esteem or personal efficacy can have long-term consequences for intellectual functioning.

Women's emotional health in old age shows more profound and lasting influences of the Great Depression than their physical health or intellectual skills. With initial health considered (as of 1930-31), the middle-class women who struggled through hard times turn out to be more self-assured and cheerful people in old age than the nondeprived ($\beta = .51, p < .05$; see fig. 4). They also appear less fretful or worrisome, less bothered by the limitations and demands of living. Hard times seem to have left them more resourceful than before and hence less vulnerable to the inevitable problems and setbacks of life. Neither socioeconomic resources nor emotional health during the early years of adulthood tell us as much as Depression hardship about the vitality and self-confidence of these women in old age.

Developmental benefits through family hardship are not apparent in the lives of women from the working class, although there may have been some undetected benefits for the wives of the artisan or skilled worker. Considering all things, the health toll of Depression life is weak among low-status women ($\beta = -.19$), and its estimate is not significant. Nonetheless, the class interaction hypothesis makes no prediction about the level of emotional costs or benefits but predicts differential effects of economic loss by class. A *t*-test of the difference between the emotional costs of Depression hardship across class strata (based on unstandardized coefficients) shows that these long-term effects differed significantly between the two strata ($p < .05$).¹⁴

¹⁴ A value of 2.44. Even more pronounced contrasts were obtained when two interviewer ratings were added to the set of old age measures, "satisfaction with life" and "self-esteem." Neither rating was available for the full sample of women in 1930-31. The new model fits the data reasonably well ($\chi^2 = 64, df = 78$), with strong negative health effects from deprivation among women in the working class ($\beta = -.33, t = 1.7$). The positive influence of hardship on the mental health of middle-class women remains essentially unchanged.

The health disadvantage of the lowest stratum has much in common with the corresponding difficulties of the survivors of extreme situations. Adaptive resources frequently made the difference between survival and helplessness (Eitenger and Strom 1973, p. 108), though extraordinary stress virtually ensured some lasting imprint of trauma. An analogy to the contrast between deprived and nondeprived women in the Berkeley middle class is provided by Judith Shuval's (1957-58) finding that some women survivors of the Nazi camps were better adapted to settlement in Israel than were controls. Definitions and appraisals of the present are shaped by past experience.

Overall, Depression loss added to the psychological disadvantage of working-class women—to their low self-esteem, feelings of insecurity, and dissatisfaction with life—but posed no such handicap for the middle class. Two groups of middle-class women, the privileged nondeprived and the economically deprived, entered the 1930s with similar inner resources. After the economic decline of the Depression, financial pressures, husband disability, and employment on a paid job were all more common in the latter group. Despite conditions of this sort, middle-class women who lived through Depression hardships occupy a position of relative advantage on psychological well-being in their later years. They claim less in the way of material goods than women from the nondeprived middle class but show greater acceptance of what they have. Gratitude and satisfaction are more common in their lives. For them, less is truly more.

ADAPTIVE POTENTIAL AND CONTROL IN OLD AGE

The reinforcing association of mastery behavior and attitudes (Bandura 1977) has particular relevance for the experience of loss among the Berkeley women in old age. A life history of mastery experience might well enable women to manage traumatic experiences produced by changes in both the environment and the human body during the later years. Rodin's (1980; see also Langer and Rodin [1976]) experimental study of nursing home residents demonstrates that "normal" declines in health, mental alertness, and activity level among the institutionalized can be slowed or even reversed when residents are placed in situations that encourage them to take some control over their lives. Levels of activity, morale, and survival were markedly higher among the aged in such settings than in "low control" situations.

This analysis moves beyond Rodin's situational emphasis to the life history that women bring to old age settings and their implications for mastery attitudes and health. We argue that some women are better equipped than others for coping with the limitations of old age, a difference

shaped by the historical interaction of life course and economic change. Unfortunately, the Berkeley archive does not include early measures of personal control. Hence, we can not assess the degree of change and stability among women along this dimension from the 1930s to the 1970s. However, we did explore connections between their *personal efficacy* in 1969–70 and their historical experience in the 1930s. Who are the women who manage to assert themselves and feel self-confident despite the physical limitations of old age?

Using the data archive for 1969, we identified appropriate measures for two constructs related to efficacy: assertiveness or self-assertion and a sense of helplessness. "Assertiveness" is measured by four nine-point ratings in the California Q sort¹⁵ (Maas and Kuypers 1974): "behaves in an assertive fashion in interpersonal situations," "values independence and autonomy," does not "give up and withdraw where possible in the face of frustration," and does not tend to "delay or avoid action." Five ratings from the California Q sort were selected as measures of "helplessness," defined as a perceived inability to influence, control, or regulate desired outcomes by selective action (see Seligman 1975): "cheated and victimized by life," "basically anxious," "vulnerable to real or fancied threat," "doubts adequacy as a person," and "self-pitying and whiny."

Assertiveness and helplessness can be viewed as elements in a sequence of action, action outcomes, and consequences. Other things being equal, one might expect assertive older women to rank lowest on a sense of helplessness, though self-assertion does not always entail the kind of success or mastery experiences that build confidence and a sense of personal control. The conceptual difference between assertiveness, a behavioral dimension, and helplessness, a subjective state, was put to a test with confirmatory factor analysis. We constructed a measurement model (not shown) that incorporated the presumed distinctions between the assertiveness and helplessness measures, and with minor adjustments (see Sorbom 1979) the model fit the data at acceptable levels ($\chi^2 = 35$, $df = 23$, $p > .05$). The four ratings of assertiveness are reliable indicators, with standardized factor loadings all exceeding .65 and no significant ties to helplessness. The other five indicators all showed reliable loadings on "helplessness" (β 's $> .55$); only "feels vulnerable" bridged the two conceptual domains with significant loadings. However, it is principally an index of helplessness (β 's = .62 and .32 for helplessness and assertiveness, respectively). Using Sorbom's (1979)

¹⁵ In the 100-item California Q sort, clinical judgments take into account the relative salience of each item within a forced normal distribution (nine categories) on each person, a procedure which avoids problems of temporal and ecological change in frame of reference. The position of an item on the distribution indicates its salience as a descriptor of the individual when compared to other items. All ratings were made by professionally trained clinicians (at least two judges per case). The average composite interjudge reliability for the Q sort as a whole is .70, with a spread from .60 to .74.

procedure, we detected only two correlated measurement errors; between "values independence" and "anxious" ($r = -.44$) and between "doubts adequacy" and "anxious" ($r = .69$).

The two unmeasured constructs, assertiveness and helplessness, are negatively correlated ($r = -.39$), and theory on their relation points to a sequence with feedback loops (see fig. 1). The efficacious experience of self-assertion diminishes feelings of helplessness; the latter experience in turn strengthens assertiveness. Multiple data points which best fit this sequential process are not available, and a model of reciprocal influence between self-assertion and helplessness could not be tested with available instruments. Given such constraints, we proceeded to estimate two separate causal models on the long-term effects of Depression hardship, one for assertiveness and the other for helplessness. These models rule out a test of whether deprivation experiences operated primarily through one of the two personality attributes. In the middle class, for example, economic deprivation may have fostered greater assertiveness, which in turn could reduce feelings of helplessness later in life. In comparison, the additional privations of the Depression in working-class families could have reinforced a victimized outlook which made assertive behavior seem pointless or unproductive.

Consistent with the interaction hypothesis, hardship experience during the 1930s is linked with assertiveness and mastery feelings among elderly women from the middle class, whereas passivity and helplessness stand out as more typical outcomes of early deprivation among lower-status women (figs. 5 and 6). Lacking early measures of personal control, we decided to include women's education (a seven-point index) as a means of indexing resource variations.¹⁶ In addition, a 1969 index of economic strain (see n. 3 above), and marital status in old age (unmarried = 1, married = 0) were included in an attempt to model the long-term objective effect of Depression hardship on social supports and material conditions.¹⁷ We assumed that both of these factors were measured without error. The causal model specifies each factor as an antecedent of assertiveness and helplessness, even though one could argue that at least financial strain is a potential cause and consequence of both assertiveness and helplessness. Whatever

¹⁶ Age was included in an earlier version of the model, but it did not influence assertiveness or helplessness. Our inability to control for 1930-31 measures of assertiveness and helplessness should not be problematic for our test of the interaction hypothesis. First, all available evidence indicates that the deprived and nondeprived did not differ in pre-Depression adaptive resources (Elder 1982). Second, emotional health, the closest approximation to personal efficacy, showed little, if any, tendency to remain the same in old age.

¹⁷ Measures of work-life patterns in the post-Depression period were also tested but had no bearing on assertiveness or helplessness. Hence, they were removed from the model to reduce collinearity. Their inclusion did not affect our main findings—the effects of economic loss.

the causal structure, the data suggest that financial strain in old age does not emerge as an important link between Depression hardship and the mastery behavior of women at this life stage.

Figure 5, on assertiveness, suggests that Depression hardship increased the risk of widowhood and economic pressure for working-class women and that both factors, as well as the direct effect of economic loss, favored a relatively passive, dependent adaptation in old age. None of these effects

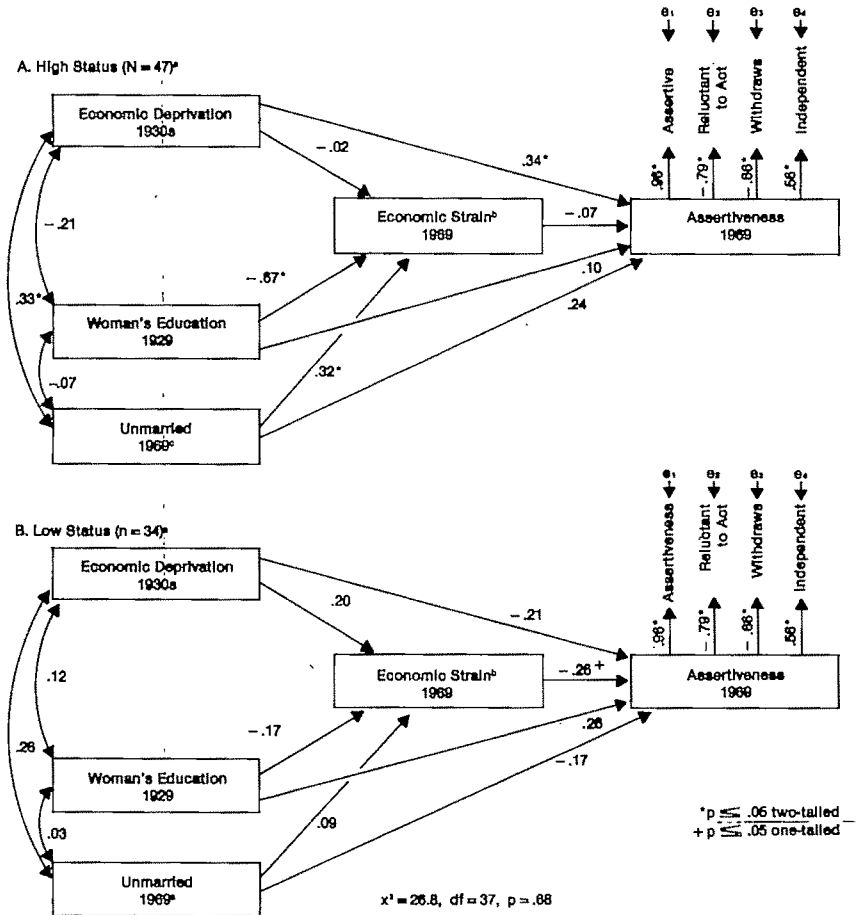


FIG. 5.—Effects of deprivation on assertiveness in old age by 1929 class (standardized coefficients). The deprivation effects were significantly different ($p < .05$) across class strata. This was tested by alternately constraining the deprivation effects to be equal and relaxing the constraint. The difference in the χ^2 in these two models was 5.8 with 1 *df*. *a*, Factor loadings were constrained to be equal across class strata. *b*, Computed as the sum of three binary interviewer ratings of general financial worry, housing quality, and neighborhood quality. *c*, Includes women who were widowed, divorced, or separated and never remarried.

is statistically reliable. Within this stratum, the most assertive women are found among the better educated who managed to reach old age with a limited history of economic disadvantage.

Women's life in the higher strata did not entail real prospects of continuing, severe hardship, unlike the common experience of women from the working class, and financial strain is clearly not a prime source of passivity in old age. Indeed, adversity, especially Depression losses and widowhood, placed middle-class women more on their own and is common to the life history of active, assertive women during the later years. As predicted by the class interaction hypothesis, the differential impact of Depression losses across class strata is statistically reliable ($p = < .05$).¹⁸

A victimized image of one's life would seem to be a natural outgrowth of the countless deprivations of the 1930s. Such an image reflects personal vulnerability, concern about adequacy, and anxiety as opposed to feelings of efficacy, hope, and self-confidence. Such concerns have roots in the harshness of working-class life in deprived situations of the 1930s. For working-class women who possessed little when the Depression arrived, the added hardships brought on by Depression loss fostered a casualty image of self ($\beta = .32$, $p < .10$; fig. 6). Note that in this stratum financial strain and widowhood also intensified feelings of helplessness.

Resourcefulness and confidence in old age represent the more common legacy of Depression adversity among middle-class women. Neither hard times in the thirties nor economic misfortune and loss of spouse during the later years managed to turn these women toward a dysphoric outlook or helplessness. Even serious health problems became just another problem to some. "I get in a little difficulty," a Berkeley woman observed (Maas and Kuypers 1974, p. 136); "I've had eight surgeries trying to correct it, but now I've just given up. I do the best I can and most of the time I get along very well." It is the middle-class women from the privileged, non-deprived sector and not the economically deprived ($\beta = -.34$, $p < .10$) who display a more victimized outlook during the later years of their life. The effects of Depression hardship varied across class strata as expected and the class difference is statistically reliable ($p < .01$; see fig. 6).

ECONOMIC CHANGE AND HEALTH IN THE LIFE COURSE: A CONCLUDING NOTE

These contrasting outcomes of Depression experience in the lives of middle- and working-class women are partly anticipated in the empirical literature on socioeconomic factors in health. This research links the risk of impaired

¹⁸ This was tested by comparing χ^2 statistics for two models—models with the structural coefficients for economic deprivation alternately constrained to be equal across classes and allowed to vary. The results yield a significant difference in the χ^2 statistic of 5.8 with 1 *df* ($p < .05$).

health to lower socioeconomic status and especially to economic setbacks in the lower strata. The direction of influence between economics and health has been questioned by analysts and the temporal span of longitudinal studies has been too short to permit estimates of the duration or persistence of economic effects. Also, research to date has viewed the economic factor largely in terms of men and their unemployment (Kasl 1979) rather than

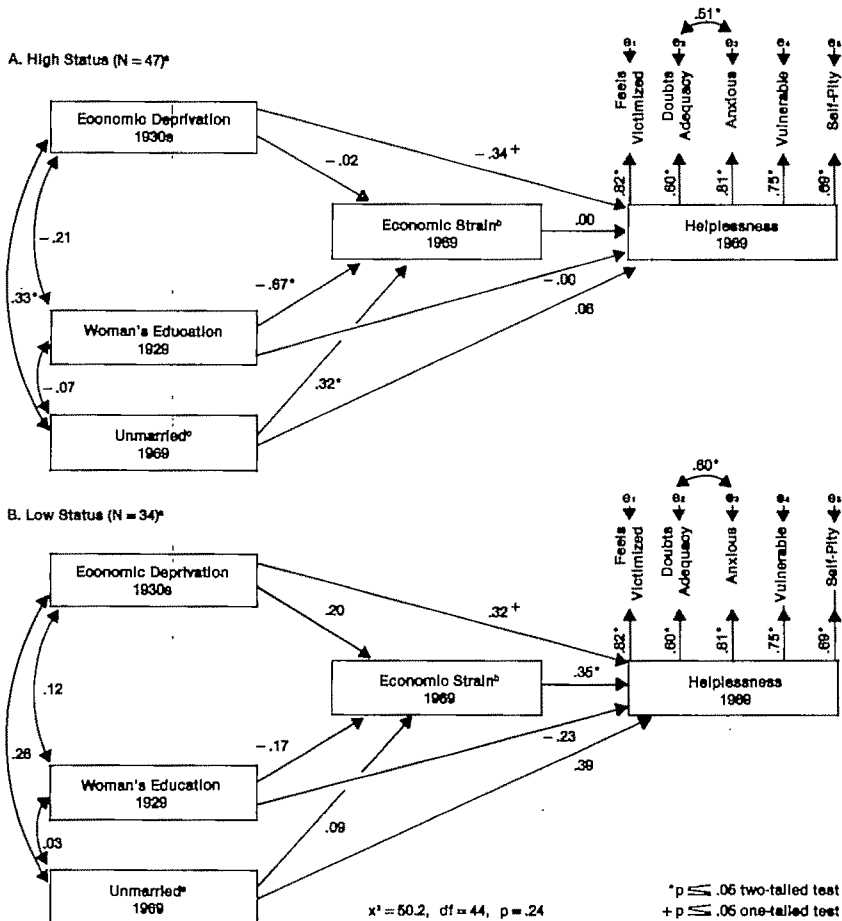


FIG. 6.—Effects of deprivation on helplessness in old age by 1929 class (standardized coefficients). The deprivation effects were significantly different ($p < .05$) across class strata. This was tested by alternately constraining the deprivation effects to be equal across class strata and relaxing the constraint. The difference in the χ^2 in these two models was 7.0 with 1 *df*. *a*, Factor loadings were constrained to be equal across class strata. *b*, Computed as the sum of three binary interviewer ratings of general financial worry, housing quality, and neighborhood quality. *c*, Includes women who were widowed, divorced, or separated and never remarried.

as an influence in the lives of women. With its focus on hard times in the lives of women, this longitudinal study traces conditions of economic deprivation to psychological health in old age. Over a span of 40 years, we see diverse imprints of the Great Depression on patterns of aging.

From the evidence at hand, different processes seem to be at work in the life trajectories of middle- and working-class women. Depression hardship increased the emotional resources, vitality, and self-efficacy of women from the deprived middle class as compared with the privileged nondeprived. For working-class women in the later years, exposure to Depression hardship generally entailed a set of disadvantages including diminished mental skills and self-confidence, lower morale, and a sense of helplessness. The consistency of this class reversal is perhaps more impressive than its size. In the working class, the modest size of the negative effect leaves open for investigation the important question of how some women from deprived circumstances managed to rise above the limitations of their world. A larger sample and more complete life records are needed to specify elements in the causal process of change and continuity and in life patterns that break with customary schedules. Social support, as a moderator of economic pressures, should be examined across the life course of marriage, parent-child relations, distant kin, and friends.

Our theoretical rationale for linking the Depression and aging experiences of the Berkeley women centers on their common ground in losses and related adaptations. We argue that women who experienced particular losses and learned to deal with them are better equipped to manage subsequent events of this type. The particular coping skills acquired in periods of hard times are not activated by tranquil stages of the life course. It is only during trying periods of decremental change, such as the later years, that coping resources are brought forth and distinguish the prepared woman from the sheltered or untested one. From this perspective, there is reason to expect the health effects of Depression hardship in the late 1930s to be weaker than those observed in old age.

Despite the daily pressures on hard-pressed families, the relative health of the Berkeley women in the late 1930s shows no reliable differences between the deprived and nondeprived in either the middle or the working class. Two types of measures were used in the analysis: (1) a simple index of behavioral impairment in performing social roles (some evidence [1936-39] or no evidence) and (2) a five-point interviewer rating on "worrisome behavior" (1936-38) identical to the worrisome ratings (1930 and 1969) used in the causal models. Of the Berkeley women, 86% were judged as showing no evidence of behavioral impairment by the end of the thirties, and the likelihood of some impairment did not vary by hard times in either social class, although it was more prevalent in the working class. A similar picture emerges from the ratings on worrisome behavior. Average scores

for the nondeprived and deprived were virtually identical in the two social classes. Though much additional work is needed on Depression influences during the 1930s, and their precise timing and form, these empirical observations generally support a situational thesis on loss and life-course development: that differential experiences with problems of loss are most visibly expressed in subsequent life situations that are typified by such events, as during old age.

To a remarkable degree, the contrasting effects of Depression hardship reported in this analysis parallel the life experience of a younger group of women who were born in 1920–21 and grew up in Oakland, California, during the 1930s. These women were young adolescents in the Depression era, and economic loss often forced them to take on adult work and domestic roles that normally would have been their parents' responsibilities. At the age of 40, when they generally had families of their own, these women were found to have long-term effects of their Depression experiences that differed by social class origin (Elder 1974, p. 242). The assumption that "smooth sailing" in a protected childhood may not develop adaptive skills was borne out in the lives of women who had grown up in nondeprived homes in the middle class. They ranked lower on psychological health and resourcefulness at the age of 40 than middle-class women who as adolescents had encountered the emotional trauma and pressure of hard times at first hand. As in the Berkeley study, the health of the Oakland women from the deprived working class was impaired relative to that of women from the nondeprived working class. Neither a privileged life nor one of unrelenting deprivation assures the inner resources for successful aging.

Social change and the normative order bring many contradictions to the life course. Some old people encounter losses without the prior experience that enables them to cope effectively with such events. The wisdom to make appropriate decisions may arrive too late, at a time when the most important choices have been made. In the words of a Berkeley woman: "It's only when you have lived through experiences and digested them that you come to acquire enough sense to know how to deal with them." According to both this personal perspective and scientific knowledge of behavioral adaptation, effective coping with personal loss through the middle years represents valuable preparation for the inevitable losses of old age.

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The Sexual Segregation of Occupations: Theories of Labor Stratification in Industry¹

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Industry-level determination of occupational segregation by sex is predicted in differing manners by split labor market, class conflict, and industrial organization theory. This paper analyzes data from 160 census industrial classifications in an attempt to assess these alternative predictions. The findings show that the degree of market power in an industry is associated positively with the level of occupational segregation, the degree of unionization is associated negatively with this level, and "classically capitalist" organization of an industry is unrelated to it. These results are interpreted as providing qualified support for the industrial organization theory.

The level of occupational segregation between the sexes in the United States is high and apparently has been so for some time (see Gross 1968; cf. Williams 1976). The subject of this paper is the way in which characteristics of different work situations influence the severity of this kind of segregation. That aspects of the "demand side" of the labor market may contribute to sex segregation has already been recognized by Blau and Jusenius, who write, "In many cases a concept of occupational assignment may be more reflective of reality than the accepted notion of occupational choice. The occupational distribution differences between men and women may to some extent reflect employer decisions to exclude women from certain level positions and/or to upgrade women more slowly than men" (1976, p. 195). What is needed is theoretical and empirical study of the extent and shape of this "structurally induced" occupational differentiation of men and women. This task is begun here with a consideration of theories of the industrial roots of occupational segregation. Thus, the present approach is distinct from much recent work that emphasizes the constraints imposed on women's occupational choices by various forces outside their work orga-

¹ Mildred A. Schwartz, Wayne Villemez, and two anonymous reviewers suggested several worthwhile changes in earlier drafts of this paper. The piece published here is a much revised version of a paper on the same topic, which was presented at the annual meeting of the American Sociology Association, New York, 1980. Requests for reprints should be sent to William P. Bridges, Department of Sociology, University of Illinois at Chicago, Box 4348, Chicago, Illinois 60680.

nizations per se, such as culture, families of origin, and families of procreation (see Oppenheimer 1970; Caplow 1964; Ireson 1978; and Moore and Sawhill 1978).

This paper attends to a limited subset of structural theories of occupational segregation, those that are concerned with the effect of industrial organization on occupational segregation. There has been a fair amount of work in sociology lately regarding the concept of industry structure; most studies have been concerned with a primary-secondary distinction (Beck, Horan, and Tolbert 1978; Tolbert, Horan, and Beck 1980; Bibb and Form 1977; Wallace and Kalleberg 1979). At the same time, it has been suggested that this twofold typology or single continuum may not be sufficient to represent the actual degree of complexity inherent in the organization of industry (Stinchcombe 1979; Mason 1976).² Thus, rather than assume a primary-secondary industrial distinction, I employ two alternative theoretical constructs. The first pertains to whether an industry is structured in the mode characteristic of classical capitalism and the second concerns the degree of market power typical of an industry. By the first, I mean the extent to which an industry is characterized by "massed" labor organized around a repetitive labor process. This conception is very similar to that proposed by Stinchcombe (1979) who propounds a scheme which includes a number of other industry types—traditional primary, bureaucratic service, professional service, etc.—which are not of direct interest here.³ The set of classically capitalist industries also overlaps considerably with Blau-

² Baron and Bielby (1980) argue a different point, that industries are the wrong units of analysis in structuralist studies of social inequality. Meeting their objections fully is impossible without launching into a lengthy theoretical debate. It ought to be pointed out that sociology has a long history of defending the ontological status of groups of all kinds, and there is no a priori reason why this practice should not be applied to groups of firms as well as groups of individuals. Thus, I maintain that the arguments presented below are correctly interpreted as statements about processes which take place at the level of industries themselves.

³ Stinchcombe's definition is as follows: "By classical capitalist industries, I mean those which formed Marx's (and Ricardo's) picture of how the labor and commodity markets relate to each other. It includes in particular the textile industry from which Marx took many of his examples and evidence. It is characterized by relatively small firms in a competitive commodity market, using unskilled labor in the labor market. That is, there is no appreciable technical or scientific apparatus of staff employees in research and development or in engineering in the managerial structure, but instead an authoritarian 'people' driving administrative apparatus. Higher productivity comes from the 'stretch out' rather than from technical improvements" (1979, p. 220). While I have been inspired by his distinction between a people-driving and a technological solution to the problem of achieving higher productivity, I believe it is unnecessary or perhaps confounding to assert that such industries also tend toward small firms and competitive markets. For example, fast food franchise restaurants in the United States seem to fit the category rather nicely, but firms in that industry are remarkably large and the degree of price competition is correspondingly low. Furthermore, what seems essential in the employment of scientific and technical personnel is that they direct their efforts toward product innovation rather than toward innovation in the process of production.

ner's (1964) categories of "machine-tending" and "mass-production" industries, that is, those found in the middle range of technology. However, in contrast to both Stinchcombe and Blauner, I believe that this industrial dimension can be explicitly applied to industries providing services as well as the production of goods. For example, little but physical surroundings can be used to distinguish the mass processing of charge account payments, catalog-store orders, and telephone calls from the mass production of shoes, bicycles, and pocket calculators.

The market-power dimension of industry structure has received more attention, particularly from economists, and refers to the degree to which firms in an industry have the potential for limiting intraindustry competition. Typical measures of this potential are provided by industrial concentration ratios and actual counts of the number of firms competing in a market (Shepherd 1970, pp. 104-5; Romeo 1975). It should be emphasized that this is an analytically independent dimension of industrial structure because it refers to the organization of the market for a firm's product rather than the conditions under which the product is made. What is distinct analytically, however, is often confounded empirically and Chandler (1977) has provided a brilliant description of how technological changes which advance an industry beyond the massed-labor stage frequently set the stage for the development of forward market integration and oligopoly. However, enough exceptions remain so that the distinction is a worthwhile one.

The connections of each of these dimensions of industrial structure to sexual segregation among occupations are important, although the exact nature of their influence is not well understood. With regard to the effect of classic capitalist organization, one hypothesis is that there should be a strong negative relationship. That is, occupational segregation represents an artificial barrier to labor mobility and, therefore, a constraint on firms' ability to reap the benefits of wage competition between different groups in the labor force. This reasoning is consistent with that which Edna Bonacich (1972, 1975, 1976) applies to the pattern of labor force organization she calls "competition." She argues that when capitalists are strong relative to majority groups in the labor force, there will be little segmentation of the labor market into noncompeting groups. Occupational segregation, or in her terminology a caste division of labor, results when there is a balance of power between majority group workers and employers. Thus, other things being equal, the more industries are organized in a classically capitalist mode, the less likely occupational segregation will be.

This position is held also by Blackburn and Mann (1979) who explain, "In any case, it is not clear that it is in the interests of capitalists to preserve women, or indeed any other group, as a separate segment of the labor

force. If women, blacks, adolescents or other secondary groups were suddenly interchangeable with white adult males, might not the latter's average wages fall, just as the former's might rise?" (p. 31).

However, a contrary point of view asserts that traditionally capitalistic work organization and its associated massed labor will have the effect of increasing the level of occupational segregation. This reasoning is based on the belief that ethnic and status-group divisions in the labor force reduce the likelihood of disruptive industrial conflict in otherwise conflict-prone settings.⁴ As early as the 1950s, Kerr and Siegal wrote that "an isolated mass can be kept from internal solidarity not only by the turnover of its membership but also by racial, religious, and nationality barriers . . ." (1954, p. 112). More recently, those working under the banner of "radical" economics have continued this line of reasoning by arguing that employers have been historically constrained by the necessity of organizing their work forces for rational profit making in a climate of sometimes latent, sometimes open, class hostility. Therefore, it is argued, they "turned to strategies designed to divide and conquer the work force." This constituted "an effort to divide the labor force into various segments so that the actual experiences of workers would be different and the basis of their common opposition to capitalists would be undermined" (Edwards, Reich, and Gordon 1975, p. xiii). By implication and by explicit statement these processes are predicted to have an impact on the division of labor by sex (Stevenson 1975, p. 97; Kessler-Harris 1975, p. 229).⁵

The second dimension of industry structure considered here, the degree of market power, also has debatable and possibly mixed effects on the degree of occupational segregation. This is particularly true because of the often noted positive effects of market power on the level of unionization in an industry. Thus, in discussing the role of market power it is necessary to consider both direct effects and indirect ones mediated by unionization.

The most widely entertained hypothesis about the direct effects of market

⁴ In specifying the factors that create the potential for industrial conflict, it is useful to refer to the literature on the interindustry potential for strikes. This literature suggests that many aspects of industry structure related to conflict potential are the same aspects that define an industry as classically capitalist: e.g., the size of the employing organization both as an organizational variable and as an ecological variable at the community level of analysis (see Britt and Galle 1974; Eisele 1974; Lincoln 1978). Furthermore, industrial technology, as defined by Woodward (1956) and Blauner (1964), is a recognized source of potential conflict and appears in numerous studies of job satisfaction and alienation (see Shepard 1971; Turner and Lawrence 1965; Goldthorpe et al. 1969).

⁵ Obviously, the radical position is centered on the assumption that ethnic, sexual, or racial divisions do tend to reduce industrial conflict. This topic is worthy of study in its own right. What is important for this analysis is the extent to which industries are organized as if these characteristics were capable of reducing the amount of nonritualized industrial disruption.

power is that they will be positive. That is, occupational segregation can be considered a form of discrimination against women, and it is believed that industries with higher levels of market power have more latitude in which to engage in costly practices like discrimination (see Shepherd 1979, p. 414; Shepherd and Levin 1973).

In some ways, the indirect effects are of greater sociological interest, for their consideration raises the issue of the role played by male workers in the maintenance of sexual differentiation. That is, it is sometimes argued that unionization represents the institutionalization of male employee privilege in an industry (see Hartman 1976, pp. 164-67; Baker 1978), and it is interesting to ask whether this will be reflected in higher levels of occupational segregation in an industry. In the logic of the split labor market developed by Bonacich, exactly such a prediction would be made. Unionization interpreted as both a symbol and a cause of male workers' power vis-à-vis employers would be expected to be accompanied by a caste division of labor.

However, it is also possible that unionization will be negatively related to occupational segregation at the industrial level. That is, many of the same authors who see segmentation of the labor force as diminishing the likelihood of industrial disruption also see unionization in the same light. The crux of these "conflict-reduction" theories is that various buffers intervene between potential and actual industrial conflict. While the buffering role may be played by ethnic and sexual differentiation, it has been attributed also, somewhat ironically, to unionization. Burawoy (1979) makes a particularly clear statement of this viewpoint: "Collective bargaining on the one hand *displaces conflict* between different agents of production from the shop floor, where it can lead to work disruptions, and on the other hand *reconstitutes conflict* in a framework of negotiations. In reorganizing conflict in this way, collective bargaining *generates a common interest* between union and company, based on the survival and growth of the enterprise" (pp. 114-15). An additional example of this logic can be found in Aronowitz (1973, pp. 219-20). Therefore, because unionization and occupational segregation by sex are alternative means to the same end, one might hypothesize a negative relationship between them.

Furthermore, we may add the possibility of a negative relationship between unionization and occupational segregation if unions are on balance a force for sexual democratization. While there can be little doubt that some unions discriminate against women, it is possible that some or perhaps many unions play an opposite role. Given the fragmentary nature of the evidence, it is impossible to make unambiguous a priori generalizations about the overall effect of unionization on sexual privilege.⁶

⁶ A reviewer has pointed out that I have ignored the question of women's membership in unions. While the overall percentage of union members who were women was quite

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Finally, unionization and occupational segregation may be negatively related if we allow for the fact that causality may run in reverse direction between them, that is, from occupational segregation to unionization. In other words, a sexually segmented labor force may so effectively prevent industrial conflict that its existence forestalls not only wild and spontaneous outbursts of worker unrest "responsible" unions would regulate but also the systematic and predictable labor militancy inherent in union organization itself.

The possible patterns of relationships among these factors are summarized in figure 1. On the basis of current theory and literature, there are grounds for expecting the relationship between classically capitalist organization and occupational segregation to be either positive or negative. The direct relationship between market power and occupational segregation is expected to be positive, but there is some question about whether the relationship is mediated completely by the level of unionization, as a measure of male worker power, or whether it has an independent existence. Market power is thought to have a direct positive influence on the level of unionization in an industry, but it is not clear whether unionization itself has a positive or negative effect on the level of occupational segregation. There is, in addition, the empirical question of the reverse influence of occupational segregation on the level of unionization; if this relationship exists, there are reasons to expect a negative sign.

In the remaining sections of this paper, data describing industries in the United States are analyzed statistically in order to clarify these relationships. As indicated below, adequate data are available to allow for separate observations on most variables (e.g., unionization, segregation, sexual composition) at the white- and blue-collar levels. The units resulting from this industry \times "collar color" classification are hereafter referred to as "industry strata groups."

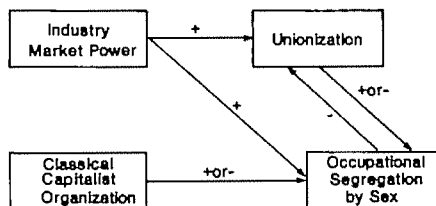


FIG. 1.—Hypothesized relationships among major variables of interest

low (20.7%) in 1970 (U.S. Bureau of the Census 1979), individual industries may have unions numerically or otherwise dominated by women. Unfortunately, usable data tabulating sex by industry by union membership are not readily available and the possibility that unionization is negatively associated with segregation because of female domination of some unions must remain in the realm of speculation.

An endeavor of this sort immediately confronts the problem that the relative simplicity of the theoretical world is lost when one contends with observations on the empirical world which have by and large been made for purposes other than testing the theories in question. In the present case, a minimum requirement is that one bring into the analysis other social factors which may be confounding the relationships discussed above.

There are three reasons why other factors need to be incorporated explicitly into this analysis. The first is that unionization reflects the power of male workers only indirectly and that industrial groups with low levels of unionization may nevertheless be populated by relatively powerful male workers. Unionization reflects only one aspect or source of workers' bargaining power, and others, such as high levels of education, need to be taken into account. At the same time, although it is not fully considered by the split labor market theory, the social resources of the "minority" group may be an important factor in their ability to resist either the imposition of a caste division of labor or exclusion. Therefore, in the subsequent analysis indicators of the social position of both men and women workers will be added to the basic model.

The second complicating factor is that occupational segregation by sex represents the *pattern* of incorporation of women into an industrial labor force, a phenomenon which is different from the *level* of incorporation of women. While the "divided labor-force" approach has called attention to the neutralizing influence of racial and sexual divisions on labor militancy, it is quite vague about the conditions under which conflict is diluted. In diminishing the potential for conflict, is it necessary for minorities to participate in a stereotyped segregated fashion, or is their sheer presence on the scene sufficient? While it might be argued that occupational segregation by sex drastically compartmentalizes the labor force and undermines a common interest, it is also possible that it blunts direct competition for jobs and reduces the level of intersex group conflict. Therefore, in this study of labor force organization by sex, it is essential that the model include a variable measuring the level as well as the pattern of female participation in an industrial strata group.

Finally, some controls are warranted by the very nature of the units of analysis considered here—blue- or white-collar industrial strata. First, there is the problem that industries are located differently in geographic and social space so that some draw their workers from the social groups with the most traditional values. For example, urban industrial strata might be expected to have much lower levels of occupational segregation than industrial strata which employ more rural individuals. Second, unionization and female employment may have other confounding causes which ought to be included in this analysis. One of these is type of industry (e.g., construc-

tion, durable-goods manufacturing, etc.) and another is whether the industrial strata group is blue collar or white collar. Thus, suitable controls for these factors will also be entered in the analysis below.

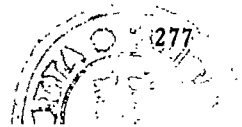
DATA AND METHODS

Unit of analysis.—Data are available to investigate these theories at the three-digit industrial level with observations existing separately for the blue-collar and white-collar levels of these industries. All in all there are complete data on 160 industries, each with two levels, so that the total number of observations is 320. Two examples of groups included in this analysis are white-collar workers in census industry 279, grain mill products, and blue-collar workers in census industry 618, vending machine operators.

Endogenous Variables

Occupational segregation.—At both the white- and blue-collar occupational levels the census bureau collects data on a vast array of detailed occupations. A detailed cross-tabulation of these occupations by industry and sex is contained in the bureau's special report tape PC(2)-7C (U.S. Bureau of the Census 1973); it allows one to calculate the degree of internal segregation in the industry strata groups defined above. For example, the individuals employed in white-collar jobs in the grain mill products industry (279) are distributed over a large number of detailed occupational titles. What is more important here is that the men and women in this group are spread differently over these detailed occupational titles and that this difference in distribution can be summarized by conventional means such as an index of dissimilarity. In fact, in 1970 there were 25,451 male workers and 10,292 female workers in this group and the occupational distributions of the sexes diverged to the extent that a dissimilarity score of .745 was obtained. A dissimilarity index was computed by similar means for all the other industry strata groups in the industry sample.

Because an important assertion of this paper is that occupational segregation can be explained as an industrial phenomenon by means of demand-side characteristics, one must ask to what extent the measure of occupational segregation used here is biased by considerations of labor supply and occupational choice. For example, some industries may have high levels of segregation among white-collar workers simply because their occupational mix in this stratum is heavily dominated by "traditional" male and female occupations. That is, we might anticipate very high levels of occupational segregation in law offices not necessarily because of the practices of



law firms, but because "external" social forces, both constraint and choice, have historically induced women to become secretaries and men to become lawyers. In order to take account of this possibility, the dependent variable in this paper is an adjusted measure of occupational segregation based on a comparison of the observed level of occupational segregation with that expected given the occupational mix of the industrial strata group in question. The details of the construction of this measure are provided in Appendix A.

Unionization.—In each of the 320 industry strata groups analyzed in this paper, the percentage of workers who are union members was taken from data published by Freeman and Medoff (1979). Their membership estimates are based on aggregations of Current Population Survey (CPS) household interviews and are provided separately for "production" and "nonproduction" workers in each three-digit industry. For the CPS estimates used in the present paper, the definition of production and nonproduction overlaps completely with the distinction between blue- and white-collar workers used.

Women's participation.—The extent of participation by women workers in each industry strata group has been measured by the simple, unadjusted proportion female which is obtained by the aggregation of individual cells in the occupation by industry by sex special report tape.⁷

Exogenous Variables

Industry market structure in this paper refers to the extent to which there are significant oligopolistic tendencies in each industry's product (or service) market. That proclivity, referred to elsewhere in this paper as market power, is measured here by the four-firm sales concentration ratio as reported in the 1972 manufacturing and business censuses (U.S. Bureau of the Census 1976*a*, 1976*b*, 1976*c*, 1976*d*). Although some have attempted to correct the official ratios for important interindustry differences in the degree of regionalism in markets, in the extent of foreign competition, and in the adequacy of census industry definitions (see Shepherd 1970), these corrections have been reported only for manufacturing industries. In broad-ranging studies such as this one, it is essential to maintain consistency of data sources across major industry groupings. Consequently, I have eschewed the use of ad hoc corrections which could be applied to only part of the data set.⁸

⁷ These percentages may also be analyzed in terms of their components, as I have demonstrated elsewhere (Bridges 1980). The reason for not doing a similar decomposition here is that this article focuses on the effects of the relative level of female employment in an industry regardless of the relative size of its components.

⁸ For some of the industries included in the analysis, business census concentration ratios were unavailable. Eight of the 25 service industries included were beset by

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Classically capitalist organization, as defined above, refers to the extent to which productivity results from the use of massed labor. As Stinchcombe states, "The main component of value added by classical capitalist manufacture is direct labor by unskilled workers . . ." (1979, p. 221). As a rough guide to operationalizing this aspect of structure, I have adopted the following criterion: classical capitalist industries are all those which have both an average firm size of 50 or more workers and an average level of fixed assets per worker of \$10,000 or less. The basic source of data on the component measures is the industry figures provided in the census of manufactures and business (U.S. Bureau of the Census 1970, 1971) with substitutions for missing data made from other relevant sources after adjustment.⁹ The classification rule is admittedly arbitrary, but it has the virtue of avoiding circularity by defining a category of industries without reference to the demographic characteristics of their work forces. In the subsequent analysis, classical capitalist organization is represented by a zero-one dummy variable; a list of industries with a score of one is provided in Appendix B. The major deficiency in the rule appears to be that it counts as classically capitalist some industries, such as shipbuilding, which have what are referred to as "engineering" technologies rather than mass-production technologies. As can be seen from inspection of the list, the category excludes nearly all of trade and services (which generally have smaller organizations), all of construction (for the same reason), and all of utilities, communication, and transportation (which have higher levels of assets).

The position of male workers relative to that of both employers and female workers is indicated by several additional variables. The incidence of high levels of education among male workers in the industry strata group is indicated by the proportion of males with more than 12 years of school. As a second indicator of the social position of male workers in the in-

missing data on this variable as were three of the 27 retail trade industries. In these instances the means of the industry-sector categories were substituted (i.e., the mean concentration of all industries in retail trade). In addition, for several of the utility industries, no concentration data were available. Shepherd (1970, p. 113-14) states, "Gas and electricity do compete strenuously for some custom, particularly for larger residential and industrial contracts; but this is only a partial rivalry among duopolists. . . . Most municipal services—water, sanitation, transit—are actual or virtual monopolies whether publicly or privately run." Following Shepherd's logic, the utility industries without concentration data were assigned concentration scores equal to the maximum concentration score "naturally" occurring in other industries, a value of 81%.

⁹ For the average-firm-size variable, the basic source of data for missing values was the county business pattern reports (U.S. Bureau of the Census 1968). A regression equation was estimated for those industries for which both sources of data were available, and the coefficients of this equation were used to generate predicted scores for the number of firms in those industries for which the only data available were from the county business pattern reports. A similar procedure was used to estimate missing data on the asset size variable except that the alternative source used was a data series published by the Internal Revenue Service (1970).

dustry strata group, the percentage of all male workers who are of prime age (25-54) has been coded. Finally, the age of female workers in the industry strata group has been considered by employing the mean age of females in years. (One advantage of the slightly different representation of male and female age is that it reduces collinearity in the model; the zero-order correlation between the two measures included is $-.271$.)

The source for each of these observations is data aggregations made from the 1-in-100 public use samples of the 1970 census (U.S. Bureau of the Census 1972). The data base for the denominators of these variables consisted of all individuals more than 15 years old in the civilian population who either were employed or had recent work experience in the industry strata group in question.

In the final statistical model, four other variables were added as controls. First, as a measure of labor force traditionalism (or, better, nontraditionalism), the percentage of workers in the industry strata group living in urban areas was coded. This measure was also drawn from the public use samples. Second, the presence of unionization and the "crude" percentage female as exogenous variables in the model necessitated a dummy variable for whether each observation pertained to a white-collar or blue-collar group in the industry. Also, as previous studies of unionization and strikes have indicated, there is a significant relationship between the absolute size of an industry and its propensity to be unionized (Britt and Galle 1974); this factor is indicated by the total number of workers in the industry strata group. Finally, two dummy variables representing the broad type of industry have been added. The first refers to whether the industry strata group is part of the traditional "industrial" sector, in the sense proposed by Fuchs (1968). In this vector, scores of one are given to observations from manufacturing, mining, construction, utilities, transportation, and communication. The second dummy variable is for service industries per se (subclassified in the census system as business and repair services, personal services, and professional services). Thus, the excluded or suppressed industry sectors in the analysis are wholesale and retail trade.

DATA ANALYSIS

Three endogenous variables have been specified above: the percentage of workers who are unionized, the percentage of workers who are women, and the degree of occupational segregation between men and women. One of the fundamental aims of the empirical analysis is to explore the relationship between unionization and segregation, which is assumed to be reciprocal. What assumptions are to be made regarding the relationships between percentage female and the other endogenous variables?

As a predictor of unionization, the percentage of female workers is subject to interpretations very similar to those applying to the degree of occupational segregation. On the one hand, greater numbers of female workers in an industrial setting, regardless of their pattern of inclusion, may introduce divisiveness into the labor force and make unionization more difficult. At the same time, to the extent that unionization reflects an institutionalization of male employee privilege, higher levels of unionization may be associated with exclusivist practices and fewer female workers. Thus, our analytic model allows for the assumption of a simultaneous reciprocal relationship between these two variables.

Theoretical analysis of the relationship between the extent of female participation and the extent of occupational segregation is quite limited. Kanter's (1977) discussion of how women become tokens in groups in which they are numerically underrepresented really applies to a different level of analysis from that employed here. It is possible that a more macrosociological analogue to tokenism does occur, however. At the industry level, small relative numbers may be not only an effect of weakness but also a cause of it. Hence the bargaining power of a minority group may be strengthened by increases in its relative numbers. In the present case, therefore, I allow for a possible effect of the percentage of female workers on the level of occupational segregation; a negative sign is anticipated for this relationship.

Before I present a formal statistical model of these relationships, one other consideration is the functional form they might take. In this analysis, each endogenous variable has been entered in logarithmic form. There are two rationales for this specification. The first is that when ratio variables have numerators and denominators that may be subject to separate errors of measurement, the error variance in the compound variable will not be a linear component of its total variance (see Stolzenberg 1975, pp. 651-52). This consideration seems to apply to both percentage union and percentage female in the present analysis. However, the difficulty is sidestepped by working with logs, because the error terms then become linear components of the total variance. A second justification for this procedure is that it simplifies the interpretation of effects involving the relative segregation measure. Because this variable has no "natural metric" after it is adjusted for expected segregation, it is difficult to interpret the effects on it of unit changes in the independent variables. When the endogenous variables are logged, however, regression coefficients between the endogenous variables can be interpreted as elasticities, or the effect of proportional change of independent variables on proportional change in dependent variables (Stolzenberg 1979). Although the exogenous variables are not entered in log form and the structural equations are of a "mixed" type, it is still legitimate

to interpret the structural coefficients of exogenous variables as the amount of proportional change in a dependent variable which would result from a unit change in an independent variable.

Equations (1a)–(1c) represent the structural form of the proposed system. The identifying assumptions are shown as zero coefficients for non-included effects of both endogenous and exogenous variables:

$$Y_1 = b_{11}x_1 + b_{12}x_2 + b_{13}x_3 + b_{14}x_4 + b_{15}x_5 + b_{16}x_6 + b_{17}x_7 \\ + b_{18}x_8 + 0x_9 + 0x_{10} + 0x_{11} + c_{12}y_2 + c_{13}y_3 + e_1; \quad (1a)$$

$$Y_2 = b_{21}x_1 + 0x_2 + 0x_3 + 0x_4 + b_{25}x_5 + b_{26}x_6 + b_{27}x_7 + b_{28}x_8 \\ + b_{29}x_9 + b_{210}x_{10} + 0x_{11} + c_{21}y_1 + c_{23}y_3 + e_2; \quad (1b)$$

$$Y_3 = b_{31}x_1 + 0x_2 + 0x_3 + b_{34}x_4 + b_{35}x_5 + b_{36}x_6 + b_{37}x_7 \\ + b_{38}x_8 + b_{39}x_9 + b_{310}x_{10} + b_{311}x_{11} + c_{31}y_1 \\ + 0y_2 + e_3; \quad (1c)$$

where:

Y_1 = ln of percent unionized,

Y_2 = ln of relative segregation index,

Y_3 = ln of percent female,

x_1 = constant,

x_2 = total industry size (millions of workers),

x_3 = dummy variable for industrial sector (manufacturing, construction, mining, transportation, utility, communication = 1),

x_4 = dummy variable for white-collar industrial strata,

x_5 = four-firm sales concentration ratio,

x_6 = percentage of male workers having more than 12 years of education,

x_7 = percentage of all workers living in urban areas,

x_8 = percentage of male workers aged 25–54,

x_9 = mean age of female workers in years,

x_{10} = dummy variable for classic capitalist industry,

x_{11} = dummy variable for service industry sector,

and

b_{ij} = coefficient of exogenous variable j in equation i ,

c_{ij} = coefficient of endogenous variable j in equation i .

The assumptions embodied in these equations were such that each model in the system is overidentified. In order to refine the initial specification of the system each equation was estimated by the use of two-stage least

squares. The models were "trimmed" by the successive deletion of effects of exogenous variables provided the following conditions were met: (i) the effect was not an essential aspect of the initial identification of the system. Thus, variables x_2 and x_3 (total industry size and dummy variable for industrial sector) were not candidates for deletion from equation (1a); (ii) the effect was smaller in size than twice its standard error. The result of these successive modifications was a more parsimonious model represented by equations (2a)–(2c):

$$\begin{aligned}
 Y_1 = & b_{11}x_1 + b_{12}x_{12} + b_{13}x_3 + b_{14}x_4 + b_{15}x_5 + b_{16}x_{16} + b_{17}x_7 \\
 & + 0\ x_8 + 0\ x_9 + 0\ x_{10} + 0\ x_{11} + c_{12}y_2 \\
 & + c_{13}y_3 + e_1;
 \end{aligned} \tag{2a}$$

$$\begin{aligned}
 Y_2 = & b_{21}x_1 + 0\ x_2 + 0\ x_3 + 0\ x_4 + b_{25}x_5 + 0\ x_6 + b_{27}x_7 \\
 & + 0\ x_8 + 0\ x_9 + 0\ x_{10} + 0\ x_{11} + c_{21}y_1 \\
 & + c_{23}y_3 + e_2;
 \end{aligned} \tag{2b}$$

and

$$\begin{aligned}
 Y_3 = & b_{31}x_1 + 0\ x_2 + 0\ x_3 + b_{34}x_4 + 0\ x_5 + 0\ x_6 + b_{37}x_7 \\
 & + b_{38}x_8 + b_{39}x_9 + b_{310}x_{10} + b_{311}x_{11} + c_{31}y_1 \\
 & + 0\ y_2 + e_3.
 \end{aligned} \tag{2c}$$

RESULTS

Figure 2 contains a summary of the results obtained from the two-stage least-squares estimation of equations (2a)–(2c). The coefficients shown in the diagram are the usual standardized regression coefficients employed by sociologists in path analysis. To supplement these, table 1 presents also the unstandardized regression coefficients which relate the endogenous variables in logarithmic form, to the other variables in the various models. In addition, the "derived" reduced form equations (see Kmenta 1971, p. 585) are shown in table 2. Here the two sets of coefficients presented are the unstandardized coefficients corresponding to those of table 1 and the partial derivatives of the "raw" (nonlogged) endogenous variables with respect to each exogenous variable. These have been evaluated at the mean of each endogenous variable and can be interpreted as the rate of change in each endogenous variable (at its mean) corresponding to a small change in each exogenous variable.

There are three major hypotheses to which these findings speak: (1) that industries with higher degrees of market power will have higher levels of sex segregation because they are better able to "afford" it; (2) that

unionization in an industry will tend to increase the level of occupational segregation as males use their bargaining leverage against females; (3) that industries which are organized in a classically capitalist mode will have higher levels of sex segregation. Referring to the findings, we see, as predicted by the industrial "organizationalists," that there is a positive effect of market power (measured by the four-firm concentration ratio) on the relative level of occupational segregation. The direct effect of market power is such that each movement of one percentage point from a fully competitive market situation to a fully monopolized one would result in a .79% increase in relative segregation between men and women. It is also noteworthy that although concentration has the expected positive influence on levels of unionization, higher levels of unionization bring about lower levels of segregation. While the effects of other aspects of male power on occupational segregation remain to be discussed, it is apparent that unionization per se has an effect on segregation which differs from that expected under the hypothesis that unionization represents only the institutionalization of

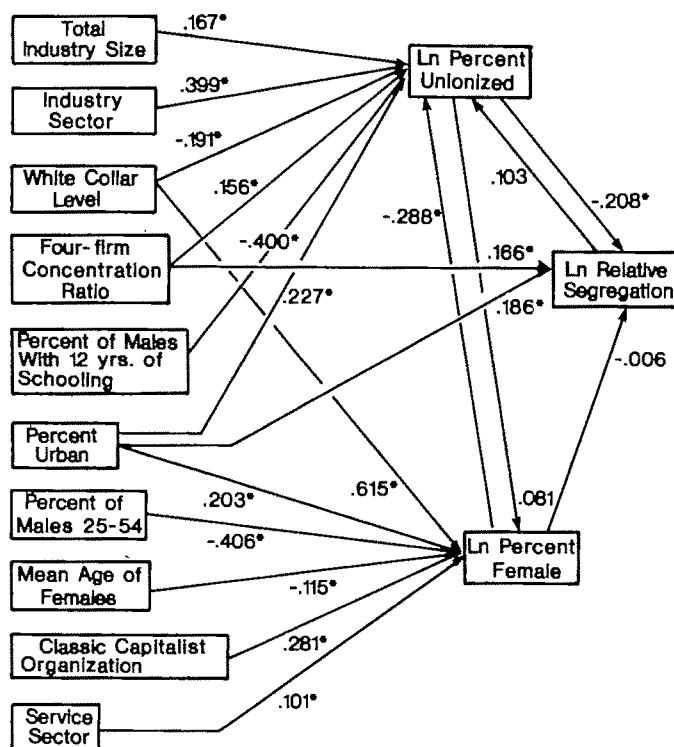


FIG. 2.—Model of female employment, occupational segregation, and unionization in 320 industrial segments. Asterisks indicate coefficients whose unstandardized equivalents are significant (see table 1).

TABLE 1
ESTIMATED COEFFICIENTS OF STRUCTURAL MODEL IN EQUATIONS (2a)-(2c)

INDEPENDENT VARIABLE	ENDOGENOUS VARIABLE					
	Ln Percent Unionized		Ln Relative Segregation		Ln Percent Female	
	b	Beta	b	Beta	b	Beta
Total industry size.....	.872*	.167
Industry sector.....	.905*	.399
White collar level.....	-.429*	-.191	1.195*	.615
Four-firm concentration ratio.....	.793*	.156	.174*	.166
% of males with more than 12 years school.....	-2.300*	-.400
% urban.....	2.170*	.227	.369*	.186	1.689*	.203
Mean age of females.....	-3.335*	-.406
Classic capitalist.....	-.031*	-.115
Service sector.....640*	.281
Ln % unionized.....	-.053*	-.208	.268*	.101
Ln relative segregation.....	1.442	.103087	.081
Ln % female.....	-.475*	-.288	-.002	-.006

* $P = < .05$.

TABLE 2
ESTIMATED COEFFICIENTS IN DERIVED REDUCED FORM

INDEPENDENT VARIABLE	DEPENDENT VARIABLE					
	Ln Percent Unionized		Ln Relative Segregation		Ln Percent Female	
	b	$\partial Y / \partial X$	b	$\partial Y / \partial X$	b	$\partial Y / \partial X$
Total industry size.....	.780	.168	-.141	-.043	.067	.019
Industry sector.....	.810	.175	-.043	-.045	.070	.020
White collar level.....	-.894	-.193	.045	.047	1.118	.323
Four-firm concentration ratio.....	.934	.202	.125	.131	.081	.023
% males with more than 12 years school.....	-2.050	-.442	.109	.114	-.178	-.051
% urban.....	1.700	.367	.275	.288	1.836	.530
% males aged 25-54.....	1.425	.308	-.069	-.072	-3.212	-.928
Mean age females.....	.013	.003	-.001	-.001	-.029	-.008
Classic capitalist industry.....	-.274	-.059	.013	.014	.616	.179
Service industry sector.....	-.114	-.025	.006	.006	.258	.075

male workers' privilege. Instead, unions on balance appear to act as forces leading to sexual democratization, not sexual segmentation.

Furthermore, in contrast to the model presented in figure 1, the reciprocal influence of segregation on unionization is quite limited and is positive. This does not support the theory developed above that occupational segregation is a safeguard against unionization; if anything, it suggests the opposite. Returning to the question how market power, measured by the concentration ratio, influences the level of occupational segregation, it is noteworthy that the reduced-form coefficients in table 2 are smaller than the structural ones, but remain positive. That is, the greater scope market power allows for invidious patterns either to arise or to survive is not effectively counterbalanced by its simultaneous encouragement of unionization and its democratizing influence.

Classic capitalist organization, insofar as it is operationalized here, has virtually no direct or mediated influence on occupational segregation. Before this concept and measure is dispatched to the "dust-bin of history," it ought to be noticed that it does have a substantial positive influence on the level of female employment in an industrial strata group. In a sense, this finding, particularly when viewed in conjunction with the strong negative impact of female employment on unionization, indicates that there was nothing incorrect in the logic of the argument about sexual stratification buffering the impact of potentially antagonistic class relationships. Instead, one interpretation is that sexual competition increases with the proportion of women in a labor force, regardless of whether that increase is associated with more or less occupational segregation. Because of this effect, our reduced-form model estimates indicate that classically capitalist industrial strata have about six percentage points fewer union members (when the effect is evaluated at the mean level of unionization) than other types of industries.

Finally, it is necessary to turn to the question how effectively other indicators of male power in the labor market predict the level of occupational segregation. Here the picture is remarkably similar to that produced for classic capitalist organization. Specifically, there are fewer females present in an industrial strata group when a higher proportion of the males are of prime age and when the average age of women in the labor force is higher. However, the age neither of men nor of women exerts much influence on the level of occupational segregation.

Stated in terms of the split labor market paradigm, the results here appear to support the argument that male workers are able to act successfully in an "exclusionist" fashion vis-à-vis female workers but are either unable or unlikely to act in support of an occupational caste system. However, this lack of direct influence does need to be considered in the light of a rea-

sonably strong correlation between the percentage of males who are of prime age and the sales concentration ratio. At the least, the influence of male market power may be confounded with the influence of industry market structure. Although a direct test has not been carried out in this analysis, industry concentration may operate as an enabling condition which permits male market power to have the predicted effect on the level of occupational segregation.

As with any statistical procedure, simultaneous equation estimation involves allegiance to untestable assumptions. However, it is perhaps reassuring to report that slight changes in the specification of the model in (2a)–(2c) did not produce major alterations in the basic findings. Furthermore, an examination of the correlation between residual error terms in these equations showed only modest association. The largest correlation was .351. Consequently, recourse to three-stage estimation did not produce any marked change of results.

One criticism which has been leveled at studies of this kind is that they rest on the assumption that occupational titles have a common meaning across industries. For example, a miscellaneous assembler in plastics manufacturing really does a different job from someone with the exact same occupational title in locomotive manufacturing. These observations are no doubt true. However, it is important to be aware of exactly where this “homogeneous occupation” assumption comes into play in the present analysis.

Until the observed segregation indices are divided by the expected ones, no homogeneity assumption has been made. In calculating observed segregation ratios within industries, the major assumption is that every array of titles actually appearing in each industry identifies groups of workers doing distinct tasks. Considering lists of titles like those in Appendix A (table A1), one might not balk at this assumption. The fact that the same titles might refer to different work in a different industry matters not, so long as only internal comparisons are being made.

However, when expected ratios are computed by the technique described in Appendix A, a homogeneity assumption is imposed. It is important to ask whether this assumption has colored these findings. Therefore, the structural model in equations (2a)–(2c) was reestimated with the observed level of segregation substituted for the relative level as an endogenous variable. A slight change in specification was made by allowing for a direct effect of collar color on occupational segregation.

The earlier conclusions were completely confirmed by this test. The effect of industry concentration on occupational segregation was positive, and the level of unionization continued to have a negative effect on segregation. All other effects retained the same signs and approximate order of relative magnitude. In addition, as expected, the level of female employment had a

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much larger effect on segregation than it did when the adjusted segregation measure was used. Not much substantive significance can be attached to this, however, because high levels of female employment in an industry may be "contaminated" by a job-choice phenomenon and reflect more than just job assignment.

CONCLUSIONS

This paper makes a basic departure from previous empirical studies of sexual segregation of occupations in its assertion that such segregation could be explained by characteristics of the demand side of the labor market as well as of the supply side. The empirical results presented above have supported this assertion. Industries do differ in the extent to which men and women are employed in dissimilar occupations; and some of these differences can be explained by structural characteristics of these industries. Although the overall pattern of results is consistent with this demand side approach, some caution is justified. One reason to be wary is that levels of explained variance in occupational segregation were relatively low. A second reason is that the results presented here are based on cross-sectional data and do not reflect changes in patterns of occupational segregation over time.

Furthermore, although industrial structure does influence the level of occupational segregation, not all aspects of structure have this capacity. For example, industries organized in the classically capitalist mode are not discernibly different from other industries in their level of segregation. Consequently predictions made about the role of occupational segregation as a conflict-reduction device have not been substantiated. On the other hand, this dimension of industrial structure was associated with a hefty increase in the absolute level of female employment (which in turn tended to depress the amount of unionization). The interpretation suggested by this pattern is that the conflict reducing benefits of demographic heterogeneity in a labor force may be most effectively realized when this heterogeneity is not buffered by a segregation of occupational roles. An interesting question for further investigation is how wage levels and male-female differences in wage levels factor into this already complicated relationship. If the so-called crowding theories are correct in asserting that occupational segregation depresses female wages and employers' total wage bills, then the benefits that direct competition between males and females brings in lowering unionization rates may have to be paid for in somewhat increased labor costs. These questions obviously lie well beyond the scope of the present analysis.

The current findings also add weight to the mounting criticisms directed

against a simple core-periphery rendition of industrial structure. It is interesting in this regard that the market power dimension of industry structure had exactly opposite effects on female participation and sexual segregation than the classically capitalist dimension did. While the latter had a substantial influence on absolute levels of female employment and virtually no influence on segregation, market power was unimportant as a predictor of female employment but was associated with higher levels of occupational segregation.

As I have pointed out, this finding is consistent with the predictions made under relatively standard theories in the economics of industrial organization. That is, levels of occupational segregation are higher in industries with more market power because the slack resources resulting from their supra normal profits can be used to offset the costs of inefficient practices like occupational segregation. What is lacking in these analyses, however, is a clear motive for segregation. Specifying that market power functions as an enabling characteristic does little to identify what social forces it enables to operate. Seen in this light, the theory of the split labor market becomes useful insofar as it singles out the supposed independent interests of majority-group workers. Therefore, the finding of a negative relationship between unionization and occupational segregation coupled with the absence of a direct main effect for other indicators of male power is meaningful—albeit in a somewhat negative way. There is no support here for the proposition that male workers' enjoyment of greater advantage contributes to higher levels of segregation. This may be because the indicators of male market power are insufficient; it may be because a proper specification of the interaction of industry market power and male privilege has not been achieved; or it may be because census industries are too-aggregated units of analysis. In any of these eventualities, nothing will be learned until new data or new analyses are brought forward. For now, the burden of proof lies with those asserting that the intransigence of privileged men is the main stumbling block to the integration of men's and women's work.

APPENDIX A

The technique for adjusting indices of occupational segregation is based on a logic of comparing, by means of a ratio, observed and expected coefficients of segregation. For each industry, the expected table is an occupation \times sex distribution which has the following characteristics. The total frequency for each occupation is the same as in the observed table, but the proportions of male and female workers in each occupation are those found in the universe of employed persons. In other words, this baseline table represents the amount of segregation which would exist if the industry kept its present

TABLE A1
DISTRIBUTION OF MALE AND FEMALE EMPLOYMENT IN CENSUS
INDUSTRY 119—GLASS AND GLASS PRODUCTS

OCCUPATIONAL GROUPS	OBSERVED DISTRIBUTION (%)		EXPECTED DISTRIBUTION (%)		ALTERNATIVE EXPECTED DISTRIBUTION (%)	
	Male	Female	Male	Female	Male	Female
Checkers, examiners, and inspectors..	6.1	40.8	11.5	28.6	11.0	28.4
Specified machine operatives.....	12.9	9.8	11.8	12.8	11.7	13.0
Packers and wrappers.....	4.9	24.0	5.5	23.8	5.2	23.2
Foreman.....	10.3	.8	9.6	2.3	9.7	2.4
Unspecified machine operatives.....	6.4	2.4	5.2	5.3	5.2	5.4
Heavy equipment mechanics.....	5.8	2.2	5.7	.3	5.8	.3
Other operatives.....	4.7	2.6	3.9	4.6	3.9	4.7
Cutting operatives.....	4.5	2.2	3.9	3.8	3.8	3.9
Freight and material handlers.....	4.9	1.0	4.8	1.0	4.9	1.1
Assemblers.....	2.2	6.2	2.4	6.1	2.3	6.0
Machinists.....	4.5	.2	4.3	.4	4.4	.4
Fork lift operators.....	3.7	.0	3.5	.1	3.6	.2
Filters and polishers.....	2.5	1.3	2.3	1.7	2.3	1.7
Janitors.....	2.8	.4	2.5	1.0	2.5	1.1
Truck drivers.....	2.1	.1	2.0	.1	2.1	.1
All other.....	21.7	8.0	21.1	8.1	21.6	8.1
Total.....	100.0	100.0	100.0	100.0	100.0	100.0
Dissimilarity index*	60.2		44.5		45.1	

* Dissimilarity index cannot be calculated directly from data presented above.

occupational distribution and if each occupation had the same sex composition in the industry as it has in the general population. By taking a ratio between the observed index of dissimilarity and the index of dissimilarity in this hypothetical population, it is possible to measure excesses of occupational segregation above those expected by virtue of the occupational distribution itself (scores greater than one) and deficits in occupational segregation (scores less than one). The analysis reported in the text of the paper uses the natural log of this ratio as an endogenous variable.

In table A1, three percentage distributions of male and female workers across occupational categories are shown for blue-collar employees in census industry 119, glass and glass products. Relative frequencies are shown for the 15 largest detailed occupation groups; these contain over 82% of the industry's blue-collar workers and each has over 2,000 incumbents. The remaining 18% of the blue-collar workers are spread over 125 detailed occupations, which are shown in the observed table to employ 21.7% of male workers and 8.0% of female workers. The first two columns in this table show the observed percentage distribution, the next two shown the expected percentage distribution as described above, and the last two an alternative expected distribution, which was not used in subsequent calculations. The sole difference between the two expected distributions is that the second was calculated so as to fit both the industry group's occupation marginal distribution and its sex marginal distribution. This was accomplished through the use of iterative proportional fitting (see Bishop, Fienberg, and Holland 1975). As can readily be observed, the two techniques yield very similar results; therefore the computationally simpler technique represented in the second pair of columns was used for the calculations on which the analysis is based.

APPENDIX B

Census Industries Coded as Classical Capitalist

258 Ordnance (nongovernment sector)	247 Optical goods and health service supplies
707 Banking	239 Scientific and controlling instruments
609 Department store and mail order	237 Mobile dwellings and campers
389 Footwear	229 Railroad locomotives and equipment
388 Tanned, curried, and finished leather	228 Shipbuilding
387 Miscellaneous plastics	227 Aircraft and parts
359 Paints, varnishes, and related products	208 Electrical machinery, not elsewhere classified

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358 Soaps and cosmetics	207 Radio, television, and communication equipment
329 Miscellaneous paper products	199 Household appliances
319 Apparel and accessories	189 Electronic computing equipment
318 Miscellaneous textile products	188 Office and accounting machines
317 Yarn, thread, and fabric mills	178 Farm machinery and equipment
309 Floor coverings, excepting hard surface	168 Miscellaneous fabricated metal products
308 Textile dyeing and finishing excepting wool	157 Cutlery, handtools, and hardware
307 Knitting mills	147 Primary ferrous metals, excepting blast furnaces and steel mills
299 Tobacco manufacturers	278 Canning and preserving
288 Confectionery and related products	
287 Bakery products	
268 Meat products	
249 Watches, clocks, etc.	
248 Photographic equipment and supplies	

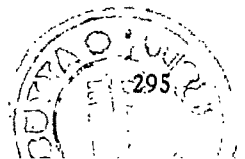
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Temporal Changes and Urban Differences in Residential Segregation: A Reconsideration¹

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Residential segregation is most commonly measured with the index of dissimilarity, an indicator that eliminates or at least minimizes the effect of group size. An asymmetric measure of isolation is described here, one which takes composition into account. Although it taps the same process in part, the measure provides a different way of approaching segregation and is found not to be a simple product of a composition variable coupled with the index of dissimilarity. Earlier conclusions about intercity differences and temporal changes in segregation between 1960 and 1970 are reconsidered. Certain features of segregation are indicated that hitherto were unappreciated. Recognition of them yields some radically different conclusions about the segregation process, shifts over time, and differences among cities. The asymmetrical approach also explains why racial and ethnic groups have different perceptions about the magnitude of and the trends in segregation.

The index of dissimilarity is by far the most widely used measure of residential segregation in contemporary sociology. Its prominence is due in no small way to the pioneering work of Duncan and Duncan (1955a, 1955b) as well as the outstanding review and comparison of segregation measures by Taeuber and Taeuber (1965). One of the strongest arguments in favor of the index of dissimilarity (D), in addition to the ease with which it can be interpreted, is that the influence of population composition and group size is eliminated or at least minimized.² By now, of course, an additional reason exists for continuing to use D ; namely, the comparisons possible because so many cities and periods have been analyzed with this measure.

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² To be sure, D is affected by group size under special circumstances (Cortese, Falk, and Cohen 1976, 1978), but this is not too serious an issue in most situations of prudent usage with reasonably large groups (Massey 1978). The conditions under which a problem arises are quite extreme and are unlikely to occur in real-life circumstances (see Taeuber and Taeuber 1976).

We reconsider the index of dissimilarity with a certain reluctance, because it is a noteworthy exception to the difficulty social science has in generating standardized measurement. Nevertheless, this is an exceptionally appropriate time to reconsider it because the analysis and interpretation of 1980 census results will soon begin. Hence, the comparisons below between D and the perspective provided by an asymmetrical composition-influenced approach should be considered now, before results for another decade are generated. As we shall see, the approach we present leads to a rather different set of conclusions about trends in segregation over time and about differences among cities and, therefore, about both the causes and the consequences of segregation.

P^* -TYPE MEASURES

One of the losers in what Peach (1975, p. 3) described as the index war that occurred between 1947 and 1955 was a set of measures proposed by Bell (1954) as modifications of indexes used by Shevky and Williams in their social area analysis (1949). These measures, which we will refer to as P^* -type measures, seek to describe the relative isolation of groups in terms of probability models of interaction among themselves and with others. As such, these measures incorporate both the uneven distribution of the groups and their relative composition in the city. In its most general form, the equation for the index is:

$$P^*_{xy} = \sum_{i=1}^n (x_i/X)(y_i/t_i),$$

where X is the total number of members of group X in the city, x_i is the number of group X in a given subarea, y_i is the number of group Y in the subarea, and t_i is the total population of the subarea. When simple isolation of one group from all others is being considered, y_i will refer to group X , that is, the index will be ${}_xP^*_x$. Also Y may refer to the entire non- X population rather than some specific other group. The subscripts preceding and following P^* refer, respectively, to the groups from which and toward which interaction is directed. For a randomly selected member of group X , ${}_xP^*_y$ gives the probability that someone else selected from the same residential area will be a member of Y . Likewise, ${}_xP^*_x$ gives the probability that someone else selected from the same residential area will be a member of group X . The computation of P^* is simple and is illustrated elsewhere (Lieberson 1980, pp. 254-56). Another way of describing P^* indexes is as the isolation experienced by members of a given group weighted by their numbers in each subarea (see Farley and Taeuber 1968, p. 956). A special feature of P^* is that essentially it is an asymmetrical measure. Even in a two-group situation, ${}_xP^*_x$ will not equal either ${}_yP^*_y$ or unity

minus that value; likewise, except when the groups are of equal size, ${}_xP^*_y \neq {}_yP^*_x$.

The main objections to P^* -type indexes appear to stem from two sources: (1) these indexes confound population composition with segregation patterns in such a way that the resulting index value heavily reflects the former; (2) they create asymmetrical measures of segregation when it is better to have a single measure for both groups. The second criticism is not the crucial one, we believe, but the compositional issue is a serious matter. Duncan and Duncan (1955b) showed that P^* was identical to the square of eta and that eta itself was closely linked to another segregation index called the ghetto index (Gh). In turn, the correlations between intercity differences in black-white segregation measured by Gh and three dependent social variables reported earlier in Jahn, Schmid, and Schrag (1947) were grossly misleading. The correlations between segregation and the dependent variables were radically reduced when composition was taken directly into account. Following up on this, Taeuber and Taeuber (1965, p. 218) found a fairly high correlation coefficient between intercity differences in their P^* indexes and population composition whereas, in contrast, there was no linkage between composition differences and D . All of this led Taeuber and Taeuber to argue that pure and simple spatial patterns should be considered without introducing the complications of composition. If composition was relevant for some problems it would be introduced as an additional variable rather than as part and parcel of the segregation index.³

The P^* -type measure has had a persistent appeal; it appears to have been rediscovered and reinvented on several different occasions. Schnare (1977, pp. 16-17), building on the work of Zoloth (1974), employs what she calls an exposure rate. Although Bell is not cited, essentially this is a P^* -type index, with an effort made also to standardize for the compositional influence on the index. Zoloth (1976, p. 282) in turn credits two different sources for the measure, Cisin (1970) and Pugh (1972). Independently, Burstein (1976) developed what he calls the dominance index, which is also a P^* -type measure that has been applied in empirical research (Hershberg et al. 1979). Coleman, Kelly, and Moore (1975, pp. 7-9), in their analysis of school segregation, likewise propose two indexes, s and r , which turn out to be P^* unadjusted and adjusted, respectively, for composition. One can see why they moved toward a P^* -type measure in their analysis of integration of the schools. It would be one matter to find an index of dissimilarity of zero, say, in a school district such as the District of Columbia, but it would be another matter to infer much integration since virtually all of the children attending public school were black. The index of dissimilarity is superb for many problems, but in others—such as the consequences of residential segregation, the actual impact of resi-

³ For a more detailed review of criticisms, see Lieberman (1981).

dential contact, and the responses of one group to another group—we are probably more interested in the actual total contact situation than in one exclusive component. As Erbe (1975) observed in her application of Bell's P^* -type indexes to the question of socioeconomic segregation among blacks and whites, "In any measurement of segregation, there are two issues: (1) the deviation from the equal distribution of populations throughout a geographic area and (2) the residential contiguity and thus likely rate of contact between persons with unlike characteristics. These two concepts are not identical" (p. 801).

Why bother with a P^* index? As we shall see, some of the claims made regarding the linkage between P^* and composition are misleading, but more crucial are the consequences of *not* taking population composition into account. Insofar as population composition actually affects the interaction between groups with the same indexes of dissimilarity, for some research problems it is not desirable to ignore this factor. Moreover, the index of dissimilarity is a symmetrical measure—there is only one D between whites and blacks in a city or between Italians and Poles, and so on. But from a compositional perspective this is not the case—the isolation of blacks from whites is not the same as the isolation of whites from blacks in the same city. The fact that this was not fully appreciated in the original paper by Bell (1954) is perhaps a cause of the underutilization of P^* . Bell had looked at only one side of the segregation picture, but if one takes composition into account it follows that the interactions and isolation of the groups are not the same at all, even in the two-group situation. Although the remaining three indexes are fixed given any one of them, the actual situation for the groups under consideration is still relevant to an understanding of both causes and consequences.⁴ This is the case even with the limited degrees of freedom operating. In other words, knowing composition, we can make certain a priori statements about the connections among the four indexes, but we cannot say which of the many possible sets of four indexes will actually be found. That is a function of the social forces generating segregation between the groups.

The index of dissimilarity, for example, between blacks and whites in 1970 was fairly similar in Columbus, Georgia, and Tucson, Arizona (61.6 and 63.6, respectively).⁵ Would one want to say that blacks were at least as isolated from whites in Tucson as in Columbus? Certainly the dissimilarity of their spatial patterns is about the same, but in terms of actual contact one would be totally amiss in drawing such a conclusion since 2.9% of Tucson's population was black whereas the figure in Columbus

⁴ Regardless of the number of groups present, ${}_sP^*_j = ({}_jP_s) (Y/X)$. In addition, in the two-group situation, ${}_sP^*_s = 1 - {}_sP^*_j$, and ${}_jP^*_j = 1 - [(Y/X) (1 - {}_jP^*_s)]$ (Lieberson 1980, p. 257).

⁵ Based on D values reported in Van Valey, Roof, and Wilcox (1977).

was 28.6%. A similar index of dissimilarity would have radically different consequences for contact between the groups in the two cities involved. With the same D , in the two cities, whites would have far less residential contact with blacks in Tucson than in Columbus (${}_wP^*_b = .024$ vs. $.156$); in contrast, blacks would tend to have far more contact with whites in Tucson than in Columbus (${}_bP^*_w = .783$ vs. $.364$).

If the problem involves consideration of what people experience in order to determine the consequences of segregation, then clearly composition is relevant and the index of dissimilarity does not tell us what they experience in the way of residential isolation. Thus cities may have the same D but with radically different consequences for interaction and isolation (see, e.g., Lieberman 1963, pp. 37-38, 134). The other special issue pertains to the forces generating a given index of dissimilarity. If composition enters into the actual interaction between the groups, trying to explain residential patterns without taking composition into account may well be an inappropriate way of approaching the central issues for the groups involved, to wit, the magnitude of contact with the other population and with compatriots.

THE INFLUENCE OF COMPOSITION AND D ON P^* -TYPE MEASURES

If the argument for taking composition into account appears valid for some problems dealing with both the consequences and the causes of residential segregation, it is reasonable to ask whether one might not simply add a compositional variable to the existing D indexes in order to combine both facets of the problem in as simple a way as possible. In other words, what does P^* accomplish that a multivariate approach using both D and composition would not accomplish? If there is no unique benefit, one could properly argue that it would be considerably less complicated to deal with these features as analytically separate—especially since one of the elements would be the venerable D index.

At the outset we know that P^* tends to vary with the composition of the community such that the population being interacted with sets a bound on the interaction potential. For example, the ${}_jP^*_i$ index ranges from a minimum of J 's proportion of the population to a maximum of unity, whereas ${}_jP^*_i$ has a potential range from zero to I 's proportion of the population. Given these bounds, even under random conditions there will be a correlation between P and composition. The influential examination of segregation indexes by Taeuber and Taeuber (1965, p. 218), for example, found a correlation coefficient of $.72$ between percentage nonwhite and the Bell index in 188 cities in 1950. If anything, their results understate the association between intercity differences in composition and P^* -type indexes. (Our basic data set, unless otherwise indicated, consists of 104

Standard Metropolitan Statistical Areas [SMSAs] for which black-white P^* -type indexes were available for both 1960 and 1970 in Schnare [1977], table B.1, and for which indexes of dissimilarity also were available for both years for the same areas in Van Valey, et al. [1977], table 1. Both sets of indexes use census tracts as the spatial unit of analysis.) Although the linear results are similar to that reported by Taeuber and Taeuber, we find an even stronger linkage when the curvilinear nature of the association is taken into account. The correlation between wP^*_b and percentage black (with both variables converted to logarithms) is .90 and .93 respectively for 1970 and 1960. Hence from 81%-87% of the variance between metropolitan areas in their wP^*_b indexes could be accounted for by composition. Of course there is no assurance that such a high association would be found in all periods or between other racial-ethnic combinations. The association is even stronger when D is also taken into account, with multiple R of $\log wP^*_b$ on D and \log percentage black being .971 and .978 in 1970 and 1960 respectively.

Despite the fact that composition and the black-white index of dissimilarity can account for about 95% of the variance among cities in their wP^*_b indexes in the two years examined, a strong case can be made for computing the P^* -type indexes directly instead of combining composition and D . This hinges on the fact that, even if we can be reasonably confident that the relative position of P^* in different areas can be determined, the actual value of P^* is not known. This is because a given combination of D and composition can yield a wide range of possible P^* values in many different circumstances. In the simple two-group situation, for a given group, say I , knowledge of its index of dissimilarity from J (D_{ij}) and its proportion of the total population (I/T) will yield the following range of possible values for iP^*_i :

$$\text{Maximum } iP^*_i = I/T + [(D_{ij}/100)(1 - I/T)]$$

$$\text{Minimum } iP^*_i = \frac{(.50 + D_{ij}/200)^2(I)}{[(.50 + D_{ij}/200)(I)] + [(.50 - D_{ij}/200)(T - I)]} + \frac{(.50 - D_{ij}/200)^2(I)}{[(.50 - D_{ij}/200)(I)] + [(.50 + D_{ij}/200)(T - I)]}$$

In the two-group situation, the maximum $wP^*_b = 1 - \text{minimum } wP^*_w$, and minimum $wP^*_b = 1 - \text{maximum } wP^*_w$. In many circumstances, the range is quite wide for the possible P^* index even if we know both the index of dissimilarity and composition. Table 3.6 in Lieberson (1981), where the formulas were originally presented, provides an illustration of the various iP^*_i indexes under various combinations. For example, if D_{ij} between blacks and whites is 70 and one of the groups, I , is 10% of the population, then iP^*_i could be from .33 to .73 (a range of .40). If I is 30% of the

population, the range of P^* is still .18. In general, the magnitude in the range of P^* tends to be greatest when D_{ij} is large and when I/T is small.

Change over Time

The P^* index is not invariant for a city even if changes in D and composition are known and taken into account. In other words, the linkage of P^* to D and composition may shift over time. In the case at hand, this is shown when the range for ${}_wP^*$ is computed for each metropolitan area in both 1970 and 1960 on the basis of relevant D_{wb} and percentage black in each period. One can determine in which quartile of the possible range ${}_wP^*$ falls in each period. In 1960, ${}_wP^*$ was in the highest possible quartile (given the D and composition of the area) in 40 of the 104 cities, whereas only seven cities were in this quartile in 1970. In contrast, table 1 shows that the number of cities in lower quartiles increased substantially in this 10-year period. When the indexes of dissimilarity and population composition were taken into account, white interaction with blacks was shown to have declined rather sharply during the period.

Other evidence supports this conclusion. An examination of black-white isolation in the 17 largest nonsouthern cities during the period from 1890 through 1930 found the values of P^* changing in the later decades even after composition was taken into account (Lieberson 1980, p. 262). For our purposes here, the indexes of dissimilarity were also computed and the actual ${}_bP^*$ indexes then compared for each period with the possible range based on the formulas shown above and the composition and D indexes known for each city. All 17 of the actual ${}_bP^*$ indexes were in the lowest quartile in each of the first four decades (with one exception in 1920). But the situation shifted significantly in the next decade, in 1930, although ${}_bP^*$ was in the lowest quartile again in 10 cities, in five cities P^* was in

TABLE 1
POSITION OF ${}_wP^*$ ON
POSSIBLE RANGE

QUARTILE	YEAR	
	1960	1970
1.....	9	13
2.....	19	40
3.....	36	44
4.....	40	7
Total...	104	104

NOTE.—Possible range based on D_{wb} and black proportion of the population for each year. Formulas shown in text.

the second lowest quartile and in two cities it was in the next to the highest quartile. Working with data presented by Farley and Taeuber (1968), one finds that in 1960 only about half of the P^* 's (six out of 13) were in the lowest possible quartile given both composition and D for 1960, whereas five were in the second quartile and two in the third. In the brief few years before an intercensal survey was taken in each of the same 13 cities, three shifted to a higher quartile on the basis of the updated composition and indexes of dissimilarity.

In conclusion, the P^* -type indexes have a distinctive meaning in terms of describing the isolation within a group and the level of interaction across group lines. Although the separate measurement of two independent variables, composition and isolation, may well generate very high multiple correlations with P^* in some cases (after nonlinearity is properly taken into account), such a procedure is not an adequate substitute. First, we would still not have the meaningful P^* values and, second, the linkages between the absolute value of P^* and these factors may change over time—even if the correlations remain high. To be sure, if one wants to account for the P^* indexes observed in various cities, any explanation would deeply involve the population composition of the areas (the role of D would be uncertain, depending on whether one wanted to view D as dependent on P^* or vice versa). Population composition cannot be ignored if P^* is the dependent variable, but still one must recognize that the actual P^* values are not merely a reflection of population composition, even if there is a strong association between P^* and composition. Insofar as the absolute value of P^* is relevant, there is no substitute for its direct measurement since the regression of P^* on D and composition is not invariant.

CONSEQUENCES FOR UNDERSTANDING SPATIAL PATTERNS

Even if one finds that one index differs from another, the ultimate test is whether the approach under consideration provides a distinctive and helpful way of understanding various features of the residential patterns. Accordingly, for both 1960 and 1970, we have compared the results obtained when the P^* -type approach is used with the patterns apparent when only D is considered. Again, we restrict our analysis to black-white isolation in the 104 SMSAs for which both P^* -type indexes and indexes of dissimilarity are available for the two years. Although once one of the P^* indexes is known in the two-group situation along with population composition, the remaining three P^* indexes are fixed since they are simple transformations of the fourth, it will be helpful to look at more than one P^* -type index in order to appreciate better the understanding which this approach provides.

Intercity Differences

In both 1970 and 1960 the average D_{bw} is quite high, about 75 in each year, but the range among the metropolitan areas is fairly wide, running from 38 to 91 in 1960 and from 43 to 91 in 1970. Under the circumstances, one can readily understand the appeal of trying to understand why areas differ so much in their indexes of dissimilarity. Bear in mind that D tends to be unrelated to population composition, which was one of the advantages seen in the measure to begin with. In this case, the correlations between D and percentage black are .006 for 1970 and $-.23$ for 1960 (still little more than 5% of the variance in D). A significant problem appears to exist; the investigator ought to account for intercity differences in segregation, but knowledge of composition does not seem to help very much.

How much of a problem exists, however, when we consider the actual differences between cities in their P^* indexes? First, the situation is asymmetrical, with the range in P^* indexes being different for blacks and whites. The indicator of white isolation, ${}_wP^*_w$, is remarkably high, the average being close to .95 in both periods (.945 and .948 in 1960 and 1970, respectively). The range is relatively restricted as well, running from .80 to .99+ in both decades. The contrast with black isolation, ${}_bP^*_b$, is striking. The mean for all of the SMSAs was .498 in 1960 and .536 in 1970, with an enormous range in both periods; ${}_bP^*_b$ varied from .08 to .84 in 1960 and from .08 to .86 in 1970. In all metropolitan areas one finds a high and relatively uniform level of white isolation when population composition is taken into account. The level of white isolation goes down very slowly with increases in the black proportion of the population; in contrast, the level of black isolation goes up very rapidly with increases in the black proportion of the population. As a consequence, the magnitude of black isolation shifts very much with black composition whereas white isolation is relatively unaffected. None of this shows up, of course, with the indexes of dissimilarity.

One can certainly ask why D_{bw} differs from area to area, but it would be a profound mistake to take these indexes as reflecting the actual isolation of the groups from one another. In effect, the variability in question is not simply the symmetrical index of dissimilarity. Instead it reflects a relatively constant level of average white isolation from blacks and a highly variable level of black isolation from whites. Rockford, Illinois, had a D_{bw} of 80.2 in 1970 whereas D_{bw} was 51.9 in Ann Arbor (see table 3 below). To be sure, some of these differences may reflect the problem of comparing tract-based indexes in different areas. However, it is clear that this wide gap in indexes of dissimilarity does not adequately describe some features of the spatial situation. Average white interaction with blacks is indeed higher in Ann Arbor, as one would expect given the D indexes, but it is not much higher:

${}_wP^*_b$ is .0572 against .0375 in Rockford. Moreover, despite the difference of about 30 points in the two indexes of dissimilarity, black interaction with whites is also only modestly lower in Rockford, ${}_bP^*_w$ being .6008 against .6772 in Ann Arbor. In contrast, although Lexington, Kentucky, has an index of dissimilarity barely lower than Rockford's, 78.3 against 80.2, given the differences in racial composition between the two metropolitan areas, black interaction with whites is much lower in Lexington, where ${}_bP^*_w$ is .4596 as opposed to .6008 in Rockford. Obviously D was not meant to describe this feature of the segregation situation, eliminating composition as it does. Hence all of this illustrates again Erbe's (1975) point cited earlier about the very distinctive function of P^* . The error occurs when researchers assume D is describing a social situation which it neither was designed to describe nor is suitable for describing. Hence, if one looks at intercity differences in segregation, the interpretation of D cannot give any information about the rates of contact, or the degree of isolation, among groups. Only when population composition is taken into account, as it is with P^* , can rates of interaction among groups, or the isolation of one group from another, be considered.⁶

Shifts over Time

Although there was relatively little change between 1960 and 1970 in the average index of dissimilarity for all of the metropolitan areas under consideration, the specific areas vary in the direction and magnitude of their changes in D_w , as one might expect. Ranking the cities by their increases in D , the upper quartile experienced an average increase of 6.74, the next quartile had an average increase of 2.14, the next experienced a small average decline, and the first quartile a larger drop in the indexes of dissimilarity (table 2). It would be a "natural" question to ask why SMSAs vary in their changes in D .

First observe that the size of the shifts in the P^* indexes are somewhat correlated with the changes in D (r between 1960-70 changes in D_w and comparable changes in ${}_wP^*_b$ is $-.55$, with 30% of the variance in one measure's change accounted for by change in the other). This modest linkage is shown categorically in table 2. Both ${}_wP^*_b$ and ${}_bP^*_w$ change in the direction suggested by the shift in D , that is, big increases in the index of dissimilarity are accompanied by decreases in the contact blacks have with whites and vice versa. However, the shifts are radically different; cities varying greatly in their changes in D did not, on the average, really differ much in their changes in average white isolation from blacks (ranging in the four quartiles from $-.015$ to $.005$) whereas they did vary in their

⁶ Some of these interarea differences in any of the indexes may reflect the problems of comparing tract-based indexes in different areas. But that is more or less constant for the different measures and hence not really relevant here.

TABLE 2
CHANGES IN P^* -TYPE INDEXES OF METRO-
POLITAN AREAS CLASSIFIED BY
CHANGE IN D , 1960-70

QUARTILE	MEAN CHANGE BETWEEN 1960 AND 1970		
	D_{bw}	${}_bP^*_{bw}$	${}_wP^*_{bw}$
1.....	-7.66	-.014	.005
2.....	-.97	-.033	.001
3.....	2.14	-.038	-.003
4.....	6.74	-.068	-.015

NOTE.—Quartiles are ordered by change in D_{bw} from decrease to increase.

changes in the average black isolation from whites (from $-.068$ to $-.014$).

The relative constancy of ${}_wP^*_{bw}$ in metropolitan areas which changed in D is supportive of the inference generated by the cross-sectional P^* analysis. One sees little change in ${}_wP^*_{bw}$ regardless of what happens in D_{bw} ; the changes are found largely in the magnitude of average black isolation from whites, with ${}_bP^*_{bw}$ rising accordingly and probably largely in connection with increases in the black proportion of the population (a force which by itself would tend to increase white contact with blacks unless checked by increased white isolation). All of this also indicates the utility of the asymmetrical approach provided by P^* -type indexes since the changes experienced by blacks and whites are clearly different in the period under consideration. None of this can be seen with a symmetrical index such as D .

Some of these processes are illustrated in table 3 for the five metropolitan areas with the largest absolute increases in the index of dissimilarity between 1960 and 1970, the five with the largest absolute declines during the same period, and the six with the least amount of change. Note that white interaction potential with blacks actually increased in Rockford, the area with the greatest gain in D , but of course the remaining four areas in the same group experienced a decline in ${}_wP^*_{bw}$ and the five areas with the greatest decline in their indexes of dissimilarity all experienced an increase in ${}_wP^*_{bw}$. Observe, however, that two of the latter areas experienced drops in black interaction with whites even though these were areas with extraordinary declines in D . Five of the areas with a stable D had decreases in ${}_bP^*_{bw}$, while one increased. Of the 10 SMSAs selected from the most extreme situations with respect to D , three had asymmetrical patterns that were opposite for blacks and whites. In Rockford, although the index of dissimilarity rose 10 points during the period and black isolation from whites increased (i.e., ${}_bP^*_{bw}$ went down), the average white interaction with blacks went up. On the other hand, although Tacoma and San Jose experienced exceptionally large declines in their indexes of dissimilarity

TABLE 3
CHANGES IN P^* EXPERIENCED BY METROPOLITAN AREAS WITH EXCEPTIONALLY
LARGE AND SMALL SHIFTS IN D_{60} , 1960-70

METROPOLITAN AREA	D_{60}		ωP^*		ΔP^*		PERCENTAGE BLACK	
	1960	1970	1960	1970	1960	1970	1960	1970
Largest increases in D_{60} :								
Rockford.....	69.5	80.2	.0300	.0375	.7027	.6008	4.10	5.90
Austin.....	63.3	73.5	.0768	.0612	.4988	.4709	12.66	10.92
Lima.....	69.6	79.5	.0480	.0271	.6317	.4922	7.06	5.45
New Orleans.....	65.0	74.2	.1481	.1214	.3325	.2679	30.80	30.96
Lexington.....	69.8	78.3	.0877	.0682	.4890	.4596	15.19	12.31
Smallest changes in D_{60} :								
Denver.....	84.6	84.7	.0149	.0159	.4210	.3686	3.39	4.09
Pittsburgh.....	74.4	74.5	.0379	.0343	.5282	.4501	6.71	7.07
Charleston, S.C.....	62.8	62.8	.1994	.1794	.3322	.3685	36.27	31.19
Chicago.....	91.2	91.2	.0265	.0295	.1575	.1365	14.31	17.64
Columbia, S.C.....	63.6	63.5	.1607	.1270	.3596	.3564	28.87	26.16
Providence.....	72.8	72.7	.0143	.0165	.8028	.7235	1.77	2.31
Largest decreases in D_{60} :								
Tacoma.....	71.2	57.4	.0180	.0282	.7737	.7434	3.27	4.50
San Jose.....	65.6	51.1	.0060	.0159	.9151	.8950	.65	1.70
Columbus, Ga.....	78.4	61.6	.1418	.1562	.3319	.3636	28.95	28.58
Albuquerque.....	70.5	51.1	.0161	.0197	.8752	.8839	1.78	2.12
Ann Arbor.....	72.6	51.9	.0368	.0572	.5129	.6772	6.83	7.61

during the decade, the frequency of black interaction with whites declined. Changes in the index of dissimilarity would not provide an adequate indicator of trends in residential isolation and would, moreover, miss differences in the relative isolation experienced by blacks and whites. As a matter of fact, one of the striking features in table 3 is that ${}_wP^*_i$ really changed by only a small amount in all 16 metropolitan areas, regardless of whether D went up or down a great deal or remained the same. The greatest change in ${}_wP^*_i$ was a little over .03 for Columbia, one of the areas with the smallest change in D_{bw} . In the first five cities, the biggest shift was less than .03 (New Orleans) and the biggest shift in the last five was about .02 (Ann Arbor). In contrast, there were changes of .10 or more in the ${}_bP^*_w$ values in Rockford, Lima, and Ann Arbor and changes of roughly .08 in Pittsburgh and Providence, two of the areas with stable D .

CONCLUSIONS

Before drawing conclusions about the role of P^* in research on segregation, a brief consideration of efforts to standardize the measure is in order. If P^* is used as a dependent variable, it would be especially important to know the role of composition in the value observed, since big differences in P^* could merely reflect compositional factors and, on the other hand, similar P^* s could occur in areas with radical differences in composition. Understandably, several of the independent proposals of a P^* -type index have also attempted to adjust the interaction measure for population composition by determining the position of the observed value along the range that is possible given the composition of the city. In our judgment, these efforts are often less than fully successful yet at the same time they give up some of the attractive descriptive qualities of P^* . Bell (1954), for example, described a revised index of isolation (I_1) which "adjusts" a P^* index by the possible range due to composition. Bell and Willis (1957) generated indexes for 66 metropolitan areas that were supposed to take into account the effect of black composition. We find, however, that the correlation with the black proportion in each area declines somewhat but does not disappear when I_1 is substituted for P^* (r is .65 and .50, respectively). The presence of this positive association may well occur because the black proportion appears in both the "adjusted measure" and the independent variable (see, e.g., the issues raised about such correlations in Fuguitt and Lieberman [1974]). It would seem pointless to give up the attractive interaction qualities of P^* for a measure which still does not fully eliminate compositional influences. This is especially the case because a direct examination can be made by regressing P^* in each area on the relevant compositional attribute. Ignoring random factors, a regression of 1.0 and a slope of zero would occur if the relevant composition measure

were the only force operating. Although Coleman, Kelly, and Moore are among those who have proposed a standardized form, they too recognize that a regression approach might be more appropriate for some problems (1975, p. 9, n. 6).

In terms of the issues raised at the outset, the results clearly indicate that the P^* -type measures provide a distinctive approach to residential isolation with a unique perspective that cannot be duplicated with the index of dissimilarity even if a compositional variable is also added. As is the case for D , the P^* indexes have an operational meaning that is attractive for interpretative purposes. Of particular importance, P^* in many cases is not invariant even when D and composition are known. The asymmetrical quality of the P^* indexes allows us to understand how the same process, residential isolation, may appear radically different to the groups involved, because, in fact, the situation and the trends are different. Indeed, we have observed periods during which black isolation from whites increased at the same time as white isolation from blacks declined. Under such circumstances, each group will generate totally different conclusions, each being correct but simply not applicable to the events experienced by the other group.

Obviously we are not recommending that D be discarded—far from it. However, in our estimation, these results make it clear that many problems and issues about the consequences (and in some cases the causes) of segregation can benefit from a P^* -type approach. If one seeks to explain why areas vary in their P^* indexes, then obviously the sticky issue of population composition cannot be ignored. But throwing out the P^* indexes is not an adequate procedure since for many problems it probably comes closer to describing the relevant intragroup and intergroup residential situation than any measure which ignores the role of population composition. Although we have illustrated the P^* indexes exclusively with data pertaining to blacks and whites, obviously the measures are appropriate for other situations, for example, ethnic contact, socioeconomic location, and the like. Indeed, this approach may help us understand some of the apparent anomalies observed by Kantrowitz (1979) and Guest and Weed (1976) in their studies of ethnic segregation trends. It may well be that very different conclusions will be drawn about the actual changes in the isolation of these groups once population composition is allowed to enter the situation.

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Male Power and Female Victimization: Toward a Theory of Interracial Rape¹

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Previous research in criminology assumes that rape is primarily an intraracial phenomenon. But empirical studies since the late 1950s have shown substantially higher rates of black offender-white victim (BW) than white offender-black victim rape. The present study tested two models of BW rape on a set of 443 rape victimizations collected by the National Crime Panel from 1973 to 1977. The normative model interprets BW rape as a correlate of increased social interaction between black men and white women. The conflict model interprets BW rape as a correlate of increased black politicalization. The results did not support the normative model and only partially supported the conflict model. Discriminant analysis showed that the characteristics of victims were unrelated to BW rapes and that BW rapes were less likely than other rapes to follow legitimate social interaction between the victim and offender. By contrast, BW rapes were no more violent than black or white intraracial rapes. Implications for a sexual stratification theory of interracial rape are discussed.

Sociologists have long been concerned with the distribution of criminal behavior by race. Given the history of black-white sexual segregation in the United States (Woodward 1955), researchers (e.g., Partington 1965; Wolfgang and Riedel 1975) have been particularly concerned with the determinants of official reactions to sexual assaults. Prior research has shown that black men accused of sexually assaulting white women receive more serious sanctions than other sexual assault suspects. But no research has examined whether interracial rapes are in fact different from intraracial rapes. In this paper I expand research on sexual stratification by race by comparing the characteristics of inter- and intraracial rape cases. My purpose is to deter-

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mine whether the behavior of rapists, like the behavior of agents of the law, depends on the victim's race.

RATES OF INTERRACIAL FORCIBLE RAPE

Paranoia concerning the protection of white women from sexual assault by black men is a legacy of American slavery that has frequently served as a focus for racist exaggerations about black men (Myrdal 1944; Curtis 1974, p. 19). This may explain why most empirical research on interracial rape is limited to questions of whether and to what extent agents of the law discriminate against black men (e.g., Partington 1965; Wolfgang and Riedel 1975). Until recently, this focus seemed justifiable. Amir's (1971) widely cited study of forcible rape in Philadelphia concluded that only 3.3% of all cases known to police in 1958 and 1960 involved black defendants and white victims. Similarly, only 3.6% of the sample involved white offenders and black victims. Literature on rape in the 1970s (e.g., Griffin 1971; Brownmiller 1975) adopted Amir's conclusion that rape was largely an intraracial phenomenon. But beginning with Reiss (1967), empirical studies have shown higher rates of black offender-white victim rape than those reported by Amir. Table 1 summarizes proportions of interracial rape by year from the empirical literature.

Table 1 includes only studies based on crimes known to police or reported in victimization surveys. Data collected from hospital records (e.g., Holmstrom and Burgess 1978) or rape crisis centers (e.g., Medea and Thompson 1974) are excluded. Two features of table 1 are worth noting. First, every study following Amir's shows a higher proportion of black offender-white victim (BW) rape than white offender-black victim (WB) rape. In several studies (e.g., Mulvihill, Tumin, and Curtis 1969; MacDonald 1971; Nelson and Amir 1975) the proportion of rapes that are BW is more than 15 times greater than the proportion of WB rapes (significant at $p < .0001$).

A second feature of table 1 worth noting is the relationship between the proportion of all rapes that are BW and year of data collection. For example, BW rapes constituted 3.2% of Amir's 1958, 1960 sample, 5.0% of Reiss's 1965-66 sample, and 10.5% of Mulvihill et al.'s 1967 sample. After 1967, the proportion of all rapes that were BW was never below 12.9% (Law Enforcement Assistance Administration [LEAA] 1979) and several studies reported proportions over 40% (Nelson and Amir 1975; Hursch and Selkin 1974; Seattle Law and Justice Planning 1975). Thus, the proportion of all reported rapes that are BW appears to have increased in the last two decades.

Interracial Rape

Researchers who have noted similar discrepancies in proportion of interracial rape for blacks and whites (e.g., Nelson and Amir 1975; Curtis 1976; Holmstrom and Burgess 1978) have generally assumed that black women are less likely than white women to report interracial rape. But the recent availability of national victimization data (LEAA 1976, 1979) makes this assumption doubtful for two reasons. First, the National Crime Panel (NCP) is based on interviews with random samples of the U.S. population rather than official records. Thus, it is less susceptible to the nonreport biases which threaten the reliability of official data (Hindelang

TABLE 1
FREQUENCY OF INTERRACIAL RAPE BY YEAR OF OFFENSE

Source	Year Collected	Sample	N	Black Offender-White Victim (%)	White Offender-Black Victim (%)
Amir (1971).....	1958, 1960	Philadelphia	646	3.2	3.6
Reiss (1967).....	1965, 1966	Chicago	459	5.0	1.0
Mulvihill et al. (1969).....	1967	17 U.S. cities	465	10.5	.3
Nelson and Amir (1975).....	1968-70	Berkeley	158	60.8	.6
Hayman and Lanza (1971)...	1969, 1970	Washington, D.C.	2,248	20.8	.4
LaFree (1979).....	1970	Indianapolis	272	24.6	.7
Agopian et al. (1974).....	1971	Oakland, Calif.	180	34.4	2.2
LaFree (1979).....	1971	Indianapolis	105	33.3	2.0
MacDonald (1971).....	1971	Denver	165	27.3	1.8
Wilson (in Curtis [1974, p. 27]).....	1972	Washington, D.C.	739	19.0	.3
Brown (1974).....	1973	Memphis, Tenn.	535	16.0	.6
Caruso (in Curtis [1974, p. 27]).....	1973	New Orleans	189	29.1	.5
Feder (in Curtis [1974, p. 27])	1973	San Francisco	578	38.4	2.7
LaFree (1979).....	1973	Indianapolis	300	21.3	1.3
National Crime Panel (LEAA 1976).....	1973	United States	69	30.4	.0
Seattle Law and Justice Planning (1975).....	1973	Seattle	305	51.1	.7
Wilson (in Curtis [1974, p. 27]).....	1973	Washington, D.C.	641	21.0	.5
Herron (in Curtis [1974, p. 27]).....	1974	Philadelphia	670	15.8	.3
Hirsch and Selkin (1974)...	1974	Denver	545	40.3	.0
National Crime Panel (LEAA 1976).....	1974	United States	113	25.7	3.5
LaFree (1979).....	1975	Indianapolis	308	22.4	1.6
National Crime Panel (LEAA 1976).....	1975	United States	116	12.9	2.6
National Crime Panel (LEAA 1976).....	1976	United States	110	21.8	.0
National Crime Panel (LEAA 1976).....	1977	United States	45	24.4	6.7

NOTE.—Mean BW offenses = 25.80, SD = 13.46; mean WB offenses = 1.45, SD = 1.599; student's $T = 8.49$, $p < .0001$.

1978).² Table 1 shows that from 1973 through 1977, the proportion of rapes that were BW reported by the NCP ranged from 30.4% (1973) to 12.9% (1975). The proportion of all rapes that were WB ranged from 6.7% (1977) to 0% (1973, 1976). Combining the yearly NCP results from 1973 through 1977, BW rapes were 10 times more likely than WB rapes.

Second, victimization data allow assessments of the victim's reasons for not reporting crime. Analysis of these data (e.g., Skogan 1977) indicates substantial similarity in the reporting behavior of black and white victims. Thus, recent empirical studies show that BW rapes are more common than WB rapes, and this difference cannot be explained by differential reporting by race.

TWO MODELS OF INTERRACIAL RAPE

Few studies have explored possible explanations for black-white differences in rates of interracial rape. An exception is Curtis (1976) who offers two tentative explanations for increasing rates of BW rape. Both explanations begin with the assumption that America is a highly stratified society with race-specific rules of sexual access.

The normative model interprets increased rates of BW rape as precursors of changing normative patterns. This explanation rests on Durkheim's (1950, p. 71) conceptualization of crime as an indispensable component in the normal evolution of morality and law. Crime prepares the way for social change and helps to determine the forms change will take. Thus, increases in rates of BW rape may precede changing normative patterns in black-white relations. For blacks, greater occupational and social equality breaks down racial barriers imposed by the sexual stratification system and makes greater black-white interaction possible. As barriers fall, blacks become more willing to seek out whites as acquaintances, friends, and sexual partners. For whites, rejection of racial prejudice increases cross-race interaction. Thus, the normative model interprets increases in interracial rape as unavoidable correlates of increases in legitimate interaction between blacks and whites.

Unfortunately, Curtis (1975, 1976) does not explain why increased interracial social interaction leads to increases in BW but not WB rape. The crux of the normative model is that greater social interaction leads to crime and, thus, anticipates social change. But the empirical studies reviewed in table 1 strongly suggest increases in BW and not WB rape.

² My argument is that victimization studies are less susceptible to bias than official sources, not that they are totally unbiased. For example, Levine (1976) argues that victimization surveys are subject to respondent, interviewer, and coding biases.

Thus, for the normative model to be viable, we must assume that black men have increased their interaction with white women but that the amount of interaction between white men and black women has remained constant.³

Curtis's second model takes a conflict approach (see Quinney 1970; Chambliss and Seidman 1971). Conflict theorists (e.g., Collins 1975, p. 282) argue that sexual access, like other scarce resources, is determined in large part by power relationships between societal subgroups. In its extreme form, during the period of American slavery, the sexual stratification system placed few constraints on sexual access to black women by white men but severe constraints on sexual access to white women by black men (Woodward 1955; Stamp 1956). The sexual stratification system continues to provide a pervasive set of sexual access rules (Heer 1974; Stember 1976). Cleaver (1968) and others (e.g., Hernton 1965; Poussaint 1972, p. 2) argue that violating these rules challenges the authority of white society. Thus, Curtis (1975, p. 78) interprets the rape of white women by black men as "the penultimate way for a black male to serve up revenge on his white male oppressor. . . ."⁴

CONTRASTING NORMATIVE AND CONFLICT MODELS

Longitudinal data on defendants' and victims' attributes and behaviors in interracial rape cases could resolve the complex issues raised here. No such data exist. But normative and conflict models also raise different expect-

³ Curtis's (1975, p. 79) argument applies only to social interaction between specific black offender-white victim dyads. But Durkheim's (1950, pp. 70-71) conceptualization of crime suggests that changing relationships between blacks and whites in general might also prepare the way for social change and serve as a pretext for BW rape. Thus, we must distinguish here between two related normative explanations. The more specific addresses the question of whether BW rape is more likely than other categories of rape to involve victims and offenders who had some prior legitimate interaction. The more general addresses the question of whether changing perceptions of normatively acceptable interracial social behavior increases rates of BW rape. My analysis only addresses the specific issue. To assess empirically the more general issue requires data on the attitudes of rapists in BW rapes. But methodological limitations of the three major criminal data sources (i.e., self-report, victimization, official) make it unlikely that valid data on this issue will ever exist. Self-reports are of limited use for serious crimes like rape, victimization data can provide only limited information on offenders, and official data reflect a highly select sample of all rapists.

⁴ The conflict model need not assume that every interracial rape represents a conscious political act on the part of the rapist. The motivations of individual offenders are no doubt more complex (e.g., Gebhard et al. 1965). However, the deviance literature (e.g., Sykes and Matza 1957) shows that awareness of unjust social relationships may provide a justification for criminal behavior. Moreover, Comer (1957) argues that radical black politics are not limited to students and political activists but may also be found among lower-class, less well-educated blacks.

tations regarding the characteristics of interracial rape. These differences relate to (1) the victim's attributes, (2) the interpersonal context of the incident, and (3) the amount of violence involved in the incident.

The normative model suggests that rates of BW rape increase with legitimate interracial contact. This model assumes attitudinal and behavioral changes in white women as well as in black men. Curtis (1974, p. 79) and others (e.g., Poussaint 1966; Nelson and Amir 1975; Brownmiller 1975, chap. 7) argue that younger, college-educated, single, white women are likely to have more liberal attitudes toward race and are therefore more likely to interact with black men and thus be victims of interracial rape. By contrast, age, education, and marital status are irrelevant to the conflict model. If BW rape represents a political attack on a sexual stratification system implemented and maintained by white males, then the offender is not primarily interested in the characteristics of the victim (apart from her race). Rather, he is interested in striking and getting away.

Both models also make different predictions about the interpersonal context of BW rapes. The normative model predicts that BW rapes frequently follow legitimate social relations between victims and offenders. It emphasizes the victim's willingness to interact with the offender and the offender's increased confidence in this interaction. Thus, the normative model predicts that BW rapes are more likely to involve (1) acquaintances, (2) incidents which occur in the victim's home, (3) incidents where the offender had a right to be at the scene of the offense, and (4) assaults by a lone offender. All of these variables suggest legitimate interaction between victim and offender prior to the rape. Also, the normative model emphasizes that the victim in BW rape often contributes to her victimization by placing herself in a vulnerable position (see Nelson and Amir 1975; Curtis 1975, pp. 80-81). Thus, for rapes which occur outside the victim's residence, the normative model suggests that BW rapes are more likely to occur at night.

In contrast, the conflict model assumes no prior interaction between victims and offenders. Thus, BW rapes are more likely to involve (1) strangers, (2) incidents which occur away from the victim's residence, and (3) incidents where the offender had no right to be at the scene. Whether an incident occurs during the night or day is irrelevant to the conflict model. And if there are political motives as well as sexual ones,⁵ incidents are more likely to include more than one offender.

⁵ Stember (1976) and Groth, Burgess, and Holmstrom (1977) argue that power and sexual gratification are closely linked for the male in consensual as well as nonconsensual sexual relations. Unfortunately, these data do not provide direct evidence of the offender's motives. Rather, the argument here is that normative and conflict models raise different expectations about the interpersonal context of interracial rape.

Finally, both models raise different expectations regarding the amount of violence likely to characterize the BW rape. Because the normative model conceptualizes BW rape as arising out of legitimate social interaction, use of a weapon and serious injury to the victim are less likely. In contrast, the conflict model conceptualizes BW rape as a violent attack on a sexual stratification system that is controlled by white males. Thus, use of a weapon and serious injury to the victim are more likely. Also, assuming that violence on the part of offenders is related to greater physical resistance on the part of victims,⁶ the normative model predicts less, and the conflict model more, victim resistance.

Prior research has not provided an explanation for black-white differences in rates of interracial rape. Given the long history of racism in this country, ignoring such questions may seem defensible. However, I justify exploring these issues on two grounds. First, women, black and white, may be as much the victims of injustice in the operation of the criminal justice system as are black men. By assuming that high rates of BW rape are explained by false crime reports (e.g., Dollard 1957, p. 169), we are discriminating against white rape victims in the same way that we discriminate against black men when we ignore the effect of race on official reactions to rape. Second, by failing to consider differences in rates of rape between two major social groups, we may be delaying a theory that explains rape not in terms of the individual motives of rapists and victims but in terms of general relationships between men and women.

In this study, I present data from a national survey which includes characteristics of victims, offenders, and incidents. My specific purpose is to provide a tentative theory of interracial rape.

DATA

Sample

Data used here consist of 453 reports of rape and attempted rape derived from the National Crime Panel survey of crime victims, undertaken by the U.S. Bureau of the Census under the sponsorship of the U.S. Department of Justice. Because the methods and procedures used in these victimization surveys have been discussed in detail elsewhere (LEAA 1976, 1979), they are described only briefly here. In these surveys, representative samples of the U.S. population were asked to report on victimization

⁶ The causal order between victim resistance and victim injury remains unclear (Bart and O'Brien 1980; Sanders 1980). But the correlations between victim resistance and injury in these data ($r = .319$; $p < .05$) show an important association between the two variables.

they suffered during the preceding six months. Data were collected from July 1973 through June 1977. The sample includes only female victims 12 years of age and older.⁷

Variables

Table 2 shows the variables. The racial composition of the victim-defendant dyad is central to the theoretical concerns outlined above. But only 10 (2.2%) of the reports involved white offenders and black victims. The small number of these cases precluded meaningful analysis, and they were excluded from the sample. Variables relate to the normative and conflict models. These include measures of (1) victim's characteristics (age, education, marital status), (2) the interpersonal context of the crime (e.g., victim-defendant, relationship, since of offense), and (3) the amount of violence involved in the incident (e.g., weapon, physical injury). Most of these variables are straightforward and require little explanation. An exception is "defendant's right," which measures whether the defendant had the right to be at the scene of the offense either as a guest or workman.⁸ In addition, I include "offense type," whether attempted or completed, and "theft," whether the incident included both theft and rape as control variables.

RESULTS

I used multiple discriminant analysis (Cooley and Lohnes 1971) to determine the extent to which measures of victims' characteristics, the interpersonal context of incidents, and amount of violence discriminated among the three racial groups (i.e., BW, WW, and BB). This analysis shows (1) the number of functions necessary to explain the information in the criterion variables, (2) the direction of relationships between discriminating variables and racial groups, and (3) the relative importance of each variable for classifying observations into one of the three groups.

Table 3 shows the discriminant function coefficients, group centroids,

⁷ Data for victims 14 years of age and older were based on victims' self-reports. Data for victims who were 12 and 13 years old were drawn from proxy respondents in victims' households (LEAA 1976).

⁸ Having the right to be at the scene of the offense as a worker has somewhat different theoretical implications than being at the scene as a guest. Both categories were combined in the data and could not be separately analyzed. Nonetheless, the two models suggest different outcomes for this variable. The normative model emphasizes that increases in BW rape are related to increases in legitimate BW social interaction. Presumably this would include employer-worker relations. The conflict model makes no such claims.

TABLE 2
VARIABLES AND SCALES

Variables and Scales	Distribution (<i>N</i> = 443) (%)
Racial composition:	
1. Black defendant-white victim (BW).....	22.6
2. White intraracial (WW).....	61.4
3. Black intraracial (BB).....	16.0
Victim characteristics:	
Age:	
0. 21 or younger.....	50.6
1. 22 or older.....	49.4
Education:	
0. High school or less.....	71.6
1. Some college or college graduate.....	28.4
Marital status:	
0. Other.....	77.0
1. Married.....	23.0
Interpersonal context of the crime:	
Victim-defendant relationship:	
0. Acquaintances.....	34.5
1. Strangers.....	65.5
Scene of offense:	
0. Private residence.....	46.0
1. Public location.....	54.0
Time of offense:	
0. Other.....	58.5
1. Victim outside residence at night.....	41.5
Defendant's right to be at scene:	
0. No right.....	89.2
1. Had right.....	10.8
Accomplice:	
0. No.....	82.8
1. Yes.....	17.2
Victim injury-defendant force:	
Weapon:	
0. No weapon.....	76.3
1. Gun, knife, other.....	23.7
Physical injury:	
0. None.....	68.2
1. Wounds, bruises, cuts, etc.....	31.8
Medical attention:	
0. Not required.....	79.0
1. Required.....	21.0
Victim resistance:	
0. No resistance or other resistance.....	65.5
1. Physical resistance.....	34.5
Control variables:	
Type of crime:	
0. Attempted.....	67.7
1. Completed.....	32.3
Theft:	
0. No.....	87.6
1. Yes.....	12.4

TABLE 3

DISCRIMINANT FUNCTION COEFFICIENTS, GROUP CENTROIDS, AND CANONICAL CORRELATIONS FOR BLACK DEFENDANT-WHITE VICTIM AND WHITE AND BLACK INTRARACIAL RAPES ($N = 443$)

Variable	Function 1	Function 2
Age.....	.028	.108
Education.....	-.071*	-.559
Marital status.....	-.076*	-.362
Victim-defendant relationship....	.692*	-.395
Scene of offense.....	.801*	.124
Time of offense.....	-.830*	.323
Accomplices.....	.066	-.200
Defendant's right.....	-.067	-.211
Weapon.....	-.098*	.434
Victim injury.....	-.059	-.307
Medical.....	-.155	.083
Victim resistance.....	.128	.018
Type of crime.....	.127	.238
Theft.....	.424*	.305
Group centroids in reduced space:		
Black defendant-white victim..	.719	-.165
White intraracial.....	-.270	-.146
Black intraracial.....	.021	.791
Canonical correlation.....	.374	.328

*Rao's $V < .05$.

and canonical correlations for black defendant-white victim, and white and black intraracial cases. The first discriminant function explained 57.48% of the variance in the means of the observations for each group.⁹ I used Wilk's λ to determine whether this function was sufficient to pick up the information in the criterion variables (Kendall and Stuart 1966). Wilk's λ was .892 after the first function had been derived ($\chi^2 = 49.26$; $P < .0001$), showing that the second function is necessary and should be included (see also the canonical correlations for functions 1 and 2).¹⁰

The group centroids in table 3 are obtained by averaging the individual discriminant scores for all cases within each group (e.g., BW). A comparison of the group centroids tells us how far apart the groups are along each dimension (i.e., function). Thus, the first function in table 3 mainly differentiates black offender-white victim (BW) from other rapes.¹¹ The sec-

⁹ In a three-group case, 100% of the variance in the means is explained by two root discriminant functions, orthogonal to each other.

¹⁰ Note that λ is an inverse measure of the discriminating power in the original variables that has not yet been removed by the discriminant functions. Thus, the larger λ is, the less information remains.

¹¹ The functions are arranged in order of decreasing importance, so that a given difference between group centroids on the second function is less meaningful than the same difference for the first function.

ond function mainly differentiates black offender-black victim rapes (BB) from other rapes. The standardized discriminant function coefficients measure the relative contribution of each variable to each function.¹² Positive coefficients indicate that cases with high values for this function (as shown by the group centroids) are more likely to be characterized by these variables. For example, "scene of offense" in table 3 has a coefficient of .801 for function 1. This means that BW rapes were more likely than WW or BB rapes to involve women who were victimized outside their homes. Negative coefficients indicate that cases with high values for this function are less likely to be characterized by these variables. For example, education has a coefficient of $-.559$ for function 2. This means that victims of BB rape were less likely than other victims to have some college or a college education.

According to table 3, black offender-white victim rapes were more likely to involve (1) public victimizations, (2) strangers,¹³ and (3) incidents which included theft. For women raped outside their homes, BW rapes were less likely to occur at night. In contrast, black intraracial rapes were less likely to involve (1) victims with some college or a college degree, (2) strangers, and (3) married women. Black intraracial rapes were more likely to involve (1) victims attacked outside their residences at night, (2) use of weapons, and (3) theft. Victim's age, presence or nonpresence of accomplices, the defendant's right to be at the scene, victim injury, whether there was medical attention, victim resistance, and type of crime did not contribute to either function.

To assess the relative importance of each variable for classifying observations into one of the three groups, I included Rao's (1952, p. 257) *V*, a generalized distance measure of dispersion.¹⁴ Table 3 shows that seven

¹² The interpretation of the standardized discriminant function coefficients is analogous to the interpretation of β weights in multiple regression. Thus, in table 3, "scene of the offense" and "time of the offense" are about twice as important as "theft" for the first function.

¹³ The theoretical consequences of differential reporting by victim-defendant relationship should not be minimized. For example, white women who voluntarily socialize with black men before a rape might be less willing to report rapes to authorities than white women victimized by black strangers. The National Crime Panel surveys reduced the possibility of such biases by preceding each interview with an earlier visit in which respondents were encouraged to recall their recent victimization experiences (Law Enforcement Assistance Administration 1972). Of course, the effectiveness of this strategy is unknown. Again, I justify using these data because they are the least susceptible to bias of any available sources (or any sources likely to be available in the future).

¹⁴ Rao's *V* evaluates each variable in terms of whether it increases discriminatory power. A variable which contains a large amount of information already included in previously selected variables may reduce discriminatory power by bringing the groups closer together. The change in *V* has a χ^2 distribution with one degree of freedom.

variables made significant contributions to classifying observations into one of the three groups. Other variables in table 3 were not very useful in discriminating among the groups. Table 4 shows the results of an analysis which included only the seven significant variables.

According to table 4, the canonical correlation was .366 for function 1 and .299 for function 2.¹⁵ A comparison of the full and reduced equations shows that the seven variables included in table 4 are capable of classifying the observations in groups about as well as the full set of variables.

Results for the reduced variable set are similar to those obtained from the full set. The first function differentiates black offender-white victim rapes from other rapes. The second function differentiates black intraracial rapes from other rapes. Victim-defendant relationship and the time and location of the offense contribute most to classifying the first function. Victim education, weapon, and marital status contribute most to classifying the second function. Theft contributes to both functions. Rapes by black men were more likely than rapes by white men to include theft.

DISCUSSION AND CONCLUSION

The normative and conflict models make different predictions about the importance of victims' characteristics and the interpersonal context and amount of violence in BW rapes. The normative model predicts that vic-

TABLE 4
DISCRIMINANT FUNCTION COEFFICIENTS, GROUP CENTROIDS, AND CANONICAL CORRELATIONS FOR BLACK DEFENDANT-WHITE VICTIM AND WHITE AND BLACK INTRARACIAL RAPES ($P < .05$)

Variable	Function 1	Function 2
Education.....	.037	.562
Marital status.....	.057	.393
Victim-defendant relationship . . .	-.720	.347
Scene of offense.....	-.827	-.236
Time of offense.....	.843	-.268
Weapon.....	.131	-.499
Theft.....	-.429	-.387
Group centroids in reduced space:		
BW.....	-.711	.118
WW.....	.254	.143
BB.....	.029	-.714
Canonical correlation.....	.366	.299

¹⁵ The canonical correlation is a measure of each function's ability to discriminate among groups. We can also interpret the canonical correlation squared as the proportion of variance in the discriminant function explained by the three groups.

tims of BW rape will be younger, more highly educated, and more likely to be single than other rape victims. Victims' characteristics are irrelevant for the conflict model. The results supported conflict predictions. The importance of age for classifying observations was similar for all three groups. That BW rapes were more likely than BB ones to involve victims with some college education and victims who were married is explained by the fact that black women generally have lower levels of education than white women and are less likely than white women to be married.¹⁶ The importance of education and marital status was similar for white women, regardless of the offender's race. Thus, speculation that increases in BW rape rates may be due to the increasing liberalization of younger, college-educated women (e.g., Poussaint 1966; Nelson and Amir 1975; Curtis 1975) appears to be incorrect.

In terms of the interpersonal context of the incident, the normative model predicts that BW rapes develop from legitimate social interaction. The conflict model predicts no prior interaction. Three of the five interpersonal context measures supported the conflict model. Specifically, BW rapes were more likely to involve strangers and victims assaulted while away from their homes. They were also less likely than other rapes to occur at night. The three most important variables for classifying BW rapes, namely, victim-defendant relationship, scene of the offense, and time of the offense, all supported the conflict model. By contrast, none of the variables that were important for classifying BW cases supported the normative model.

The normative model assumes that BW rapes originate in prior legitimate social interaction between victim and offender. By contrast, the conflict model assumes that BW rapes represent violent attacks on a sexual stratification system dominated by white males. Thus, the normative model predicts less violence and the conflict model predicts more violence in BW cases. Neither model explained the results. Instead, I found no differences between racial groups with regard to physical injury, medical attention, or victim resistance. The only distinction between the cases lay in whether the offender was armed. Offenders in BB rapes were more likely than other offenders to be armed. A plausible explanation for this finding is simply that black offenders anticipate that the chances are greater of encountering an armed black woman than an armed white one. This explanation may be particularly important for rapes that occur in inner-city, high-

¹⁶ Median years of school completed were 10.3 for black females and 12.2 years for white females in 1970 (United States Bureau of the Census 1975, p. 330). Of all black women, age 18-24, 21% were enrolled in colleges in 1975, compared with 24% of all white women for the same age group (United States Bureau of the Census 1979, p. 90). For 1970, 49% of all black women over 14 years of age were married, compared with 62% of all white females (United States Bureau of the Census 1979, p. 109).

crime areas, where many black women are routinely exposed to threats of violence (see Bordua and Lizotte 1979).

Taken together, the results suggest that the normative model must be rejected and the conflict model substantially modified. The conflict model begins with the assumption that sexual access, like other commodities that are both valued and scarce, is determined by power relations within a highly stratified sexual market. In America, this market has historically been controlled by white males. Conflict theorists (e.g., Hernton 1965; Poussaint 1966) and black leaders (e.g., Cleaver 1968; Jones 1966) have conceptualized BW rape as a violent political act associated with growing black anger at inequality and oppression. But the results do not fully support this interpretation. True, BW rapes were not characterized by any marked differences in victim attributes. And black men who raped white women tended to pick strangers, in public places, during the day. But BW rapes were no more violent than other rapes. Moreover, if the motive in these rapes was solely a desire to strike back at the sexual stratification system, it is unclear why black offenders, whether their victims were white or black, were more likely than white offenders to rob as well as to rape victims. Recent research by Stember (1976) may provide a partial explanation.

Stember claims that an important aspect of sexual stratification by race is the extent to which American society has promoted the belief that white women (or lighter-skinned black women) represent the ideal of sexual attractiveness. Stember and others (e.g., Fishman 1961; Hoetink 1967) argue that black men in white-dominated societies are widely influenced by this myth.¹⁷ For men, both black and white, sexual gratification and power are closely related (e.g., Stember 1976; Groth et al. 1977). But because of a long history of sexual stratification by race in America, the meaning of interracial rape (and seduction) is different for black and white men. For some blacks, white women represent not only a highly stratified and repressive social system but also freedom, self-worth, and power (see Hernton 1965; Hippler 1974; Curtis 1975, p. 79). Thus, Poussaint (1972, pp. 95-96) argues that interracial contacts "may give a few black men a special feeling of worth and manhood."

In sum, prior research clearly documents white fear of black sexuality with regard to white women (e.g., Myrdal 1944; Schulman 1974) and shows that compared with other racial combinations, black men who assault white women receive more serious official sanctions (e.g., Wolfgang and Riedel 1975; LaFree 1980). Results from empirical studies reviewed

¹⁷ The myth of superior white female sexual desirability may also explain the greater frequency of BW than WB marriages (Heer 1974).

here strongly suggest that BW rapes are, in fact, more common than WB rapes and that this difference is not explained by differential reporting. The normative view that BW rapes are preceded by legitimate social interaction between black men who are more confident and white women who are more open to interaction with blacks cannot explain this difference. The conflict model is accurate in that the victim's characteristics were unrelated to racial composition and that BW rapes were not generally preceded by legitimate social interaction. But contrary to the conflict interpretation of BW rape as a violent political act, BW rapes were no more violent than other rapes. A more plausible explanation for the greater frequency of BW than WB rapes is that a white-dominated sexual stratification system has enshrined the white female as a symbol of sexual attractiveness, freedom, and power. The major difference between BW rapes and other rapes is less political than strategic: because the social system continues to separate black men from white women, offenders in BW cases select victims with whom they have had no prior social contact.

The failure of social science to evaluate critically issues of race and sex, while perhaps motivated by good intentions, has delayed the development of theoretical explanations for interracial sexual behavior. Longitudinal data on the characteristics of victims and offenders would help resolve the issues raised here. Further research on the motivations of rapists might also be relevant (see Fisher and Rivlin 1971). Although this analysis was limited to contemporary America, the implied relationships would apply to all societies which include a sexually segregated underclass (see Daniel 1968; Welsh 1969; Inglis 1974). More generally, relationships between rape and power may be relevant to other male power struggles (e.g., Rangan 1974; Scacco 1975).

If we assume that America is characterized by race-specific rules of sexual access, then white fear about the rape of white women by black men and the greater frequency of BW than WB rape become two different aspects of the same phenomenon. White males set this sexual stratification system in place by promoting the white female as the standard of sexual desirability. White men have attempted to maintain this system by illegal violence against black men (e.g., Inverarity 1976) and by differential application of the law (e.g., Wolfgang and Riedel 1975; LaFree 1980). For their part, black men have been influenced by white definitions of sexual attractiveness (Fishman 1961; Cavior and Howard 1973). The empirical evidence shows that BW rapes are more common despite harsh punishments for the sexual violation of white women. Fundamentally, the struggle between black and white males represents an age-old conflict where men from dominant groups attempt to protect their sexual property from

subordinate-group men, while simultaneously ignoring the victimization of subordinate-group women. Women, black and white, have historically been the victims of this struggle.

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Race, Class, and the Perception of Criminal Injustice in America¹

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The results of two research literatures, one dealing with criminal sentencing and the other with public rankings of crime seriousness, have raised doubts that conflict exists in American society about issues of criminal justice. This paper offers a different and more direct approach to this issue by analyzing public *perceptions* of criminal *injustice* and by assessing the capacity of conflict theory to explain them. Our analysis is based on a national survey, and it focuses on the race, class, and status positions of the respondents, with class position measured in neo-Marxian terms. Three major findings are (i) that black Americans are considerably more likely than white Americans to perceive criminal injustice; (ii) that regardless of race, members of the surplus population are significantly more likely than members of other classes to perceive criminal injustice; and (iii) that class position conditions the relationship of race to the perception of criminal injustice, with the division between the races in these perceptions being most acute in the professional managerial class. These findings constitute substantial evidence that race and class conflict exist with regard to issues of criminal injustice, and that neither kind of conflict can properly be understood without consideration of the other. Implications for Marxist and non-Marxist criminologies are indicated.

The meaning of criminal justice, like the meaning of crime, is symbolic and variable. Criminal justice is *symbolic* in that the criminal law and its enforcement are expected to embody fundamental principles in our society.

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Criminal justice is *variable* in that these symbols and principles are varied, both in their legal conception and in their public perception. Philosophers, from Plato and Aristotle to Rawls (1971), have fought these facts by searching for those elusive principles that could give the idea of justice absolute meaning. They have failed (Nettler 1979, pp. 28-31) because conceptions and perceptions of justice are determined in large part by the times, places, and positions in the social structure from which they are derived. This does not mean that there are no standards by which criminal justice can be measured or that criminal justice itself is standardless. However, it does suggest that our standards of criminal justice, such as "equality before the law," will be ambiguously and impermanently conceived, as well as variably perceived.

For example, Nettler (1979, p. 30) is able to identify three common meanings of equality (numerical, proportional, and subjective equality), all of which are regarded as important in some form of legal behavior. Nettler goes on to draw an important connection between social change and criminal justice by noting that the specific meaning of equality as applied in the pursuit of justice often varies with consideration of the vantage point. Thus in an earlier era when laws involving the crimes of women and children developed in this country, an *assumption* of subjective equality (or equity) dictated that unequal treatment be mandated by legislation for what were then regarded as *unequal* members of society. Although these laws often encouraged differential leniency, and although they were justified as "protective," they perpetuated symbolically an unequal status for women and children. Today as women, and sometimes children, are recognized as equals, equal treatment (numerical equality) is also demanded, and the older laws in turn are now more likely to be regarded as "discriminatory."

Sociologists interested in studying criminal justice have responded typically to the situation above by trying to make the existing law explicit and by then attempting to expose disparities between legal statutes and legal behavior. An example of this approach includes the large and expanding literature that looks for systematic links between the status characteristics of criminal offenders and the sentences they receive. Much of this work has included an effort to test a conflict theory of crime. The results are inconsistent; while there are a variety of studies that find little (e.g., Bernstein et al. 1977a) or no (e.g., Chiricos and Waldo 1975) relationship between status characteristics and sentencing, there are also a few that find these relationships to be substantial (e.g., Swigert and Farrell 1977; Lizotte 1978) and others that find these relationships to be contingent on particular circumstances (e.g., Hagan, Bernstein, and Albonetti 1980). In response to such studies, conflict theorists increasingly concede

the point that relationships between status characteristics and court outcomes are neither as large nor as consistent as is frequently assumed. Indeed they have argued that such findings are actually quite consistent with the theory, suggesting that "discrimination is not only in principle unnecessary but also likely to be counterproductive in contributing to the demystification of the structure of legal control" (Turk 1976, p. 292) and that "ruling classes have a general interest in promulgating and reproducing the stability of the social order *as a whole*, and . . . an important way of achieving this is by somehow ensuring that the severity of sanctions *ought* not significantly to be correlated with social class" (Beirne 1979, p. 378).

One might be tempted to infer from the research above, and from the reactions of conflict theorists to it, that the symbols of criminal justice are reasonably well intact in American society. This kind of inference is further encouraged by a different kind of research designed to measure crime seriousness. The pioneering work in this area is Sellin and Wolfgang's (1964) scaling of 141 criminal offenses in terms of public rankings of their seriousness. Part of the initial motivation for this work was probably Sellin's (1938) early appreciation of the potential role of conflict in the definition of crime. More recently, researchers (Rossi, Waite, and Berk 1974; Thomas 1976; Newman 1976) interested in testing conflict hypotheses about crime have used similar measurement strategies to determine if significant class, race, and other divisions exist in public rankings of crime seriousness. The implicit assumption has been that if group-linked variations in public rankings were found that diverged from the penalties imposed by criminal statutes, this would signal a way in which the criminal law is used unjustly by some groups to impose their interests and values on others. However, research in this tradition has failed to find substantial group-linked differences in public ranking of the seriousness of crimes. Indeed, from Sellin and Wolfgang to Newman, the data suggest rather remarkable levels of societal agreement, and it has been demonstrated more recently that these seriousness rankings correlate well with the severity of sentences received by convicted offenders (Blumstein and Cohen 1980). Conflict theorists have responded by arguing that these findings actually are consistent with the theory. Thus Turk (1976, p. 289) suggests that "a high degree of consensus on the relative seriousness of crimes can be interpreted as evidence of the successful control and mobilization of ideological resources so as to minimize the divergence of beliefs otherwise expected to result from social inequalities." Similarly, Michalowski and Bohlander (1976) speak of a "false consensus" (p. 97) and argue that "sub-groups tend to agree upon what is serious, not because this definition serves their own or society's interests but because it is diffi-

cult to understand a social creation such as crime outside the social context in which it is presented" (p. 105).

The arguments above make conflict theory difficult to test (see also Hagan and Leon 1977). More important, however, this form of argument may be interpreted as conceding more than it should. Uniformity in criminal justice decision making cannot be demonstrated to exist apart from an unambiguous knowledge of what the legal standards of these decisions should be. Knowledge of these legal standards requires a clear division between legal and extralegal variables in these decisions. Bernstein et al. (1977b, p. 367) identify three obstacles to this division:

First, there is considerable variation from one jurisdiction to another in the procedural law that stipulates what factors are legal versus those that are extra-legal in criminal justice decisions. Second, what is specified in a statute as legal for one stage of criminal justice processing may not be legal for another stage, e.g., community ties (flight risk) is generally a legal consideration for pre-trial release status decisions, but not for plea bargaining or sentencing decisions. Third, some variables ordinarily placed in the 'legal' category (e.g., prior record of convictions) may themselves have resulted from some combination of consideration of legal and extra-legal variables in some prior processing.

Similarly, no widespread "false consensus" can be known to exist apart from a demonstration that various publics in some more general way actually perceive the operations of our criminal justice system to be just. A correspondence between public estimations of crime seriousness and legal sanctions is only a part of this requirement (Turk 1976). It is not clear that critics of conflict theory have claimed more than this.

Meanwhile, the irony is that no one has analyzed public *perceptions* of criminal *injustice* and assessed the capacity of conflict theory to explain them. Claims of criminal injustice represent a significant form of conflict because they raise fundamental doubts about the operational meaning of principles (like equality before the law) on which American society is based. This paper seeks to determine how race and class positions, among other variables, may organize the expression of this conflict. In doing this, we will make the further point that conflict criminologists have not yet operationalized the concept of class in a meaningful way or developed a clear understanding of the relationship between class and race in the determination of crime-related conflicts. An analysis of race, class, and the perception of criminal injustice is an appropriate place to begin this work.

THE DATA

The data for this study come from a national survey designed to measure perceptions of, and experiences with, local, state, and federal courts, as

well as more general attitudes toward the administration of justice and legal actors. The study was conducted by Yankelovich, Skelly and White Inc. (1977), under the direction of the National Center for State Courts, for the Law Enforcement Assistance Administration of the U.S. Department of Justice. The study is based on interviews conducted between October and December 1977 with a single-stage, stratified, replicated random sample of Americans 18 years of age and older (for a more detailed discussion of sampling procedures, see Inter-University Consortium for Political and Social Research [IUCPSR] [1980], pp. ii-v). Analyses reported in this paper involve 1,049 persons for whom a head of household was known to be employed and on whom occupational information was available, and 59 persons for whom a head of household was known to be *unemployed* and *not* retired or a student or a housewife. These constraints were imposed so that all persons considered in this study could be classified in terms of class position. We have introduced these data here because they are central to the following discussion of the concepts and indicators that organize our research.

A NEO-MARXIAN MEASURE OF CLASS

Although the concept of class is central to conflict theories of crime and criminal justice (Chambliss and Seidman 1971; Quinney 1980; Taylor, Walton, and Young 1973; Turk 1969), little attention has been given by conflict criminologists to the actual operationalization of this concept. Recently, however, Marxist scholars have begun to operationalize the concept of class for more general purposes. No single prevailing Marxian measure of class has emerged from this work, and there are several reasons to doubt that one will. First, Wright (1980, p. 323) points out in a recent review essay that "while class is perhaps the pivotal concept within Marxist theory, Marx himself never provides a systematic definition of class." Second, there appears to be widespread agreement among Marxists that class categories cannot be made exact. Sweezy (1953, p. 24) makes this point when he suggests that "it would be a mistake to think of a class as perfectly homogeneous internally and sharply marked off from other classes. Actually there is variety within the classes; and one class sometimes shades off very gradually and almost imperceptibly into another." Third, there seems to be no urgency among Marxists to come to a resolution of this issue. Braverman (1974, p. 409) suggests that such an issue "cannot always be solved neatly and definitively, nor, it should be added, does science require that it must be so solved."

The approach we take to the measurement of class is informed by Marxian ideas. For example, it begins with a premise that probably all Marxists

accept: that classes are to be defined in relational rather than in gradational terms (Wright 1980, p. 325). The point is that the classes we consider are not merely conceived as "above" or "below" one another; instead, these classes are defined in terms of their social relation to one another, with each class representing a common structural position within the social organization of the relations of production.

We use three conceptual criteria to distinguish four class positions. It is important to note that our interest here is in these class positions more than in the particular types of individuals who fill them: for example, it matters not whether our respondents are heads of households or their spouses; what matters is their common position in the class structure, which is identified in terms of occupational and employment information on the head of household.² The three conceptual criteria considered are ownership of the means of production, relationship to the means of production, and relationship to labor power. The four class positions are employers, the professional-managerial class, the working class, and unemployed workers, whom, following Marx, we call the surplus population. The relationships between the conceptual criteria and class positions are presented in table 1. A discussion of each class in terms of these criteria follows.

TABLE 1
CRITERIA FOR CLASS CATEGORIES

	Ownership of Means of Production	Relation to Means of Production	Relation to Labor Power	Distribution (%)
Employers.....	Owner	Accumulative	Buys labor	7.7 (85)
Professional-managerial class (PMC).....	Nonowner	Reproductive	Sells labor	27.8 (308)
Working class.....	Nonowner	Productive and unproductive	Sells labor	59.2 (656)
Surplus population.....	Nonowner	Unproductive	Unable to sell labor	5.3 (59)

NOTE.—Numbers in parentheses are *N*'s.

² To test empirically the theoretical assumption that for our purposes a spouse occupies the same class position as his or her "head of household," we created a measure indicating whether the respondent's occupational designation (e.g., housewife) was of a different category from that of the head of household, and we included this new variable in our regression equations. The resulting effect parameters are nonsignificant. For example, when added to eq. (3a) of table 5 below, this variable is related to the perception of criminal injustice as follows: $r = -.02$; $b = 1.00$; $B = -.03$; $F = .951$. In other words, our assumption seems to be correct: respondents with occupations different from their heads of household nonetheless appear to join the latter in their perceptions of criminal injustice.

Employers are first of all *owners* of the means of production, as indicated by an answer of yes to a survey item, "Do you now own a business?" Second, their relationship to the means of production is classified as *accumulative* on the basis of our knowledge that they are owners of a business and that the occupation of the head of household falls within one of these survey categories: official, proprietor, manager, or professional. Use of this occupational information eliminates from our employer category persons who own their own businesses but are engaged in clerical, sales, service, or other kinds of skilled and unskilled laboring activities, such as crafts; in other words, these are categorized as members of the petty bourgeoisie who have very little opportunity to accumulate capital. (We do not consider the latter class in our analysis because Marxists typically argue that "the petty bourgeoisie represents a remnant from an earlier era of capitalist development and, as a class, . . . is progressively becoming less important" [Wright and Perrone 1977, p. 43].) Third, employers are identified as *buyers of labor power*, an inference drawn from the fact that it is virtually impossible to be an official, proprietor, manager, or professional and to own your own business without employing labor power beyond your own. Defined in this way, employers constitute 7.7% ($N = 85$) of our sample. This figure is within a few decimal percentage points of a class category of the same name measured in a somewhat different way by Wright (1978; also Wright and Perrone 1977). We refer to the occupants of this class position as "employers" rather than "capitalists" because most of the persons in this type of category employ fewer than 10 workers (Wright 1978, p. 1370).

Our second class category, the professional-managerial class (PMC), is drawn from the work of the Ehrenreichs (1978; see also Walker 1978), who define this class "as consisting of salaried mental workers who do not own the means of production and whose major function in the social division of labor may be described broadly as the reproduction of capitalist class relations" (p. 12). This definition is readily translated into our criteria. First, members of the PMC are identified as *nonowners* on the basis of their answers to the question about business ownership. Second, they are identified occupationally as officials, proprietors, managers, or professionals and (as nonowners) assumed to be in a *reproductive* relationship to the means of production; that is, they are assumed to be much more involved in the coordination and control of the social relationships necessary for capitalist production than in the creation of these relationships or in the actual production of commodities. Finally, members of the PMC are classified as *sellers of labor* on the basis of an item indicating that they are employed and our previous knowledge that they are not owners

of businesses. Defined in this way, the PMC constitutes 27.8% ($N = 308$) of our sample. The Ehrenreichs' own estimate is that this class makes up 20%–25% of the population.

Our third class category is the working class. Members of this class obviously are also *nonowners* of the means of production, as indicated by the business ownership item. The relationship of this class to the means of production is classified as *productive or unproductive* on the basis of its members' identification occupationally as being involved in skilled and unskilled laboring activities, including being craftsmen and foremen, or in clerical, sales, and service work. While the former occupations are productive in the sense of creating use value, the latter are not. Finally, workers are identified as *sellers of labor* in the same manner as the PMC, the difference being that the workers' occupations are not reproductive in character. Defined in this way, the working class constitutes 59.2% ($N = 656$) of our sample. The Ehrenreichs' (1978, p. 14) own estimate is that 65%–70% of the American population is working class. However, when Wright (1980, p. 368) operationalizes the Ehrenreichs' definition, he arrives at an estimate of the working class as 63.1% of the population, while Wright's own operational definition of working class membership yields an estimate of 41.6%. He then points out deficiencies in his own definition and concludes that a more meaningful estimate might be 55% (p. 368n.). Our figure is close to this.

Surprisingly, recent Marxist operationalizations of class structure (e.g., Wright 1978; Wright and Perrone 1977; Robinson and Kelly 1979) have not included consideration of *unemployed workers*. This is ironic because Marx (1912, pt. 7) attached great importance to this "surplus population" in developing his General Law of Capitalist Accumulation, and because Marxists more generally (see Zeitlin 1967, chap. 4) have regarded this grouping as crucial to the changing character of class relations. Marxian criminologists have recently noted the significance of this category in terms of the problems of regulation presented by this class and the consequences of these regulatory problems in terms of crime and criminal-justice-related conflicts. Carter and Clelland (1979, pp. 98–99; see also Spitzer 1975; Balkan, Berger, and Schmidt 1980, chap. 3) make these points in arguing that "the very system which creates this 'surplus population' . . . generates massive discontent. . . . Because such shifts are a basic threat to the political economy of capitalism, the capitalist state is faced with the problem of the regulation of surplus labor. This problem is accommodated by the increasing intervention of formal systems of social control, criminal and juvenile courts, into the private sociocultural life of the marginal fractions of the working class."

Implicit in this statement is a use of the concept of class that is closer

to the purposes of Marx than are the recent quantitative studies of income inequality. For Marx, the concept of class was first and foremost an analytic tool for explaining structural change in the capitalist system. Crucial to the prospect of structural change in the Marxian scheme is the emergence of class consciousness. A common response of the surplus population to their presumed position vis-à-vis the criminal justice system could be taken as one important indication of class consciousness. Quinney (1980, p. ix) argues that "the future of criminal justice will be determined by changes in the objective conditions of the last stages of capitalism and by a rising consciousness, especially among that expanding portion of the working class now relegated to the status of surplus population." We will be particularly attentive in this paper to the possibility of the surplus population showing signs of being such a "class for itself."

Thus the last category in our classification consists of unemployed workers, whom we will call the surplus population. The surplus population consists of *nonowners* of the means of production, as indicated by our business ownership question; they are *unproductive*, as indicated by the fact that they are not actively involved in any occupational category; and they are *unable to sell their labor*, as indicated by their designation as unemployed. Defined in this way, the surplus population constitutes 5.3% ($N = 59$) of our sample.

RACE AND OTHER CONFLICT VARIABLES

All conflict theorists have not focused on class alone (e.g., Collins 1975; Turk 1977). For this reason we will consider other status positions of our respondents in terms of their race, education, income, region, age, sex, and area of residence as well. Among the latter variables, race is expected to be salient. The history of conflict between black and white Americans is manifest, as is the connection of that conflict to crime issues (e.g., Wolfgang and Cohen 1970) and criminal justice (e.g., Wolfgang and Reidel 1973). Our sample is limited to black and white Americans, and race is coded as a dummy variable, with blacks coded one. The assumption is that blacks are much more likely than whites to perceive criminal injustice.

Collins (1971, 1975, 1979) makes the case for conflict among educational status groups. He argues that education transmits status culture by providing instruction in styles of speech, dress, taste, manners, and so forth that are later translated into occupational placement: "The mechanism proposed is that employers use education to select persons who have been socialized into the dominant status culture: for entrants to their own managerial ranks, into elite culture; for lower level employees, into an attitude of respect for the dominant culture and the elite which carries it"

(1971, p. 1011). From this perspective, education should diminish perceptions of criminal injustice. We have operationalized education as a quasi-credential scale in which 1 = postgraduate college, 2 = graduated college, 3 = some college, 4 = graduated high school, 5 = some high school, and 6 = grade school or less.

Income is an obvious conflict variable, often accorded a dependent position in conflict analyses. In this study, income is treated as an independent variable, and it is measured as total family income. Families with incomes of less than \$5,000 are coded 8, those with incomes to \$7,499 are coded 7, those with incomes to \$9,999 are coded 6, and so on in \$5,000 intervals up to an income of \$35,000 or more, which is coded 1. To the extent that income influences the perception of criminal injustice, lower incomes might be expected to increase this perception.

The South is a unique region in American society, with its own particular history of conflict, aggravated by its rapid development under agrarian capitalism, its Civil War defeat and subsequent underdevelopment, and its "rotten borough" role in the national Democratic party (Hamilton 1972). From the time of early lynchings to the reinstatement of capital punishment, this conflict has been widely thought to be expressed in forms of criminal injustice. Region is coded as a dummy variable, with southerners coded one, to see if persons from this part of the country perceive more criminal injustice than other Americans.³

What makes age status a unique conflict variable (Friedenberg 1965; Flacks 1971) is that for each respondent it constantly changes. The earlier periods of this change are more likely to produce conflict than later periods, as reflected by the fact that younger adults are more likely than older adults to have experience with the criminal law (e.g., Richards 1979). Respondents 18–20 years old are coded 11, those 21–24 years old are coded 10, and so on in four-year intervals to those 65 years of age and older, who are coded 1.

It is difficult to know how the conflict of the sexes might influence the perception of criminal injustice. In contrast to the perception of their experiences with other institutions, women are widely thought to receive lenient treatment from agencies of crime control (Simon 1975; Adler 1975; Hagan, Simpson, and Gillis 1979). It may be men, then, who are most likely to perceive criminal injustice, and in this dummy coding, men are therefore coded one.

³ Assuming that region might also interact with race, we estimated eq. (3a) of table 5 below separately for blacks and whites. This procedure failed to reveal signs of an interaction effect, and therefore the possibility of this interaction is not discussed further in this paper.

The last of our conflict variables considers whether the respondents in our survey reside in the central cities of America or in more peripheral areas of the country. This distinction draws importance from Frank's (1967) neo-Marxian work on the development of underdeveloped areas. Although Frank (1978, 1979) has focused most of his attention on the world stage, he also notes that "this contradictory metropolitan center-peripheral satellite relationship, like the process of surplus expropriation/appropriation, runs through the entire world capitalist system in chain-like fashion from its uppermost metropolitan world center, through each of the various national, regional, local, and enterprise centers" (1967, p. 10). Following the premise that one consequence of this metropolitan-periphery relationship of domination and dependency is a colonialization of attitudes, we will assume that conflicts about issues like criminal injustice are more likely to be explicit in the center than in the periphery of American society. That is, we expect that residents of central cities will be more likely than persons in the periphery to perceive criminal injustice. For this reason, central city residents are coded one, and residents of the periphery zero, in our analysis.⁴ We can turn now to the measurement of our dependent variable.

THE PERCEPTION OF CRIMINAL INJUSTICE

The perception of criminal injustice has a variety of components, a number of which are considered in our data. We begin with 10 problems of criminal injustice indicated in table 2: two of them involve law enforcement officials/police, four involve the courts, two involve juries, and one each involves lawyers and judges. All of the problems relate directly or indirectly to the justness of decisions reached in the criminal justice system, with special attention given to the experiences of economic and ethnic minorities with problems of equality before the law. Each of the problems was referenced in the interviewing to the respondent's state, and the problems were presented in random order. Respondents first ranked the seriousness (no problem, small, moderate, serious, very serious) of each problem and then the frequency of its occurrence (never, once in a while, some of the time, most of the time, all the time). We have combined the seriousness and frequency rankings of each item into a single variable

⁴ In these data, a central city consists of 50,000 inhabitants or more in the 1960 or 1970 census, or of two cities "having contiguous boundaries and constituting, for general social and economic purposes, a single community with a combined population of at least 50,000, with the smaller city having a population of at least 15,000" (U.S. Department of Commerce 1977).

ranging from 1 to 10, and we have combined the 10 problem variables into a 100-point scale. In this paper we will consider these variables separately and as a scale. A preliminary issue, however, is whether this combination of variables can be said to constitute a scale.

Table 2 presents correlations between each of the 10 variables and the 10-variable scale, with and without correction for inclusion of the specific variable considered. The lowest of these correlations is .60. A factor analysis of these variables resulted in one factor (with loadings presented in table 2) that explained 100% of the common variance of the variables; this indicates that the scale is unidimensional. Measures of the reliability of the scale are very encouraging: Cronbach's (1951) α is .91 and Heise and Bohrnstedt's (1970) omega is just over .91. Measures of internal or trait validity are also highly supportive: Heise and Bohrnstedt's validity measure ρ is over .95. Their measure of invalidity, ψ^2 , is effectively zero (.003), indicating that there is very little variation among the index items due to causes or factors other than the one underlying dimension being measured. These variables, then, clearly do form a scale of the perception of criminal injustice. Means and standard deviations for this scale and all other variables considered in this study are presented for the full sample and within class categories in table 3.

TABLE 2
PERCEIVED CRIMINAL INJUSTICE SCALE

Item*	r^\dagger	Corrected r^\dagger	Factor Loadings§
Law enforcement officials/police who do not treat poor suspects the same as well-to-do suspects (PCI1)76	.69	.73
Law enforcement officials/police who do not represent a cross section of the community in which they work (PCI2)68	.60	.65
Courts that disregard a defendant's constitutional rights (PCI3)74	.67	.72
Juries that do not represent a cross section of the people in the community (PCI4)71	.63	.67
Juries that are biased and unfair when it comes to deciding cases (PCI5)75	.69	.73
Lawyers who do not treat their poor clients the same as their well-to-do clients (PCI6)72	.65	.69
Judges who are biased and unfair (PCI7)76	.70	.73
Courts that do not treat poor people as well as well-to-do people (PCI8)80	.74	.78
Courts that do not treat blacks and other minorities the same as whites (PCI9)76	.68	.74
Courts that are influenced by political considerations (PCI10)68	.60	.63

* Each item is ranked separately on a Likert Scale by respondents in terms of seriousness as a problem and frequency of occurrence (see text).

† Correlations between the item scores and the total scale score.

‡ Correlations between item scores and total scale score corrected for inclusion of the item considered (see Scott 1968, p. 219).

§ Factor loadings based on a principal-factor analysis with iterations.

TABLE 3
MEANS AND STANDARD DEVIATIONS OF VARIABLES USED IN REGRESSION EQUATIONS

	Full Sample	Employers	PMC	Workers	Surplus Population
Race.....	.10 (.30)	.02 (.15)	.07 (.25)	.11 (.31)	.22 (.42)
Education.....	3.46 (1.24)	3.04 (1.28)	2.69 (1.18)	3.82 (1.05)	3.95 (1.34)
Income.....	4.23 (1.77)	3.17 (1.54)	3.33 (1.55)	4.59 (1.56)	6.59 (1.85)
Region, South.....	.35 (.48)	.34 (.48)	.32 (.47)	.37 (.48)	.36 (.48)
Age.....	7.12 (2.55)	5.82 (2.40)	7.05 (2.43)	7.24 (2.59)	8.07 (2.33)
Sex.....	.49 (.50)	.45 (.50)	.52 (.50)	.49 (.50)	.41 (.50)
Residence, central city.....	.38 (.49)	.29 (.46)	.36 (.48)	.40 (.49)	.44 (.50)
PCI1.....	6.47 (1.96)	6.52 (1.93)	6.21 (1.89)	6.52 (1.99)	7.20 (1.79)
PCI2.....	5.34 (1.82)	5.27 (1.58)	5.39 (1.85)	5.26 (1.81)	6.08 (2.11)
PCI3.....	4.89 (2.01)	4.75 (2.10)	4.69 (1.88)	4.90 (2.00)	6.10 (2.34)
PCI4.....	5.30 (1.95)	5.12 (1.83)	5.34 (1.99)	5.27 (1.93)	5.64 (2.06)
PCI5.....	5.22 (1.88)	4.79 (1.67)	5.05 (1.84)	5.26 (1.87)	6.20 (2.10)
PCI6.....	6.60 (1.91)	6.54 (1.82)	6.29 (1.86)	6.67 (1.90)	7.44 (2.08)
PCI7.....	5.57 (1.89)	5.54 (1.94)	5.43 (1.74)	5.56 (1.91)	6.47 (2.04)
PCI8.....	6.27 (2.13)	6.18 (1.94)	6.09 (2.15)	6.29 (2.14)	7.16 (1.90)
PCI9.....	5.78 (2.21)	5.21 (1.92)	5.71 (2.17)	5.77 (2.24)	7.03 (2.08)
PCI10.....	6.50 (1.96)	6.53 (1.80)	6.37 (1.93)	6.49 (1.98)	7.22 (1.87)
PCI scale.....	57.93 (14.56)	56.46 (12.26)	56.57 (14.92)	57.99 (14.35)	66.58 (15.04)

NOTE.—Standard deviations are in parentheses.

EQUATIONS

In the first part of our analysis we estimate the following regression equations:

$$\text{Perceived Criminal Injustice} = a + b_1 \text{ Race}, \quad (1)$$

$$\text{Perceived Criminal Injustice} = a + b_1 \text{ Workers}, \quad (2)$$

and

$$\begin{aligned} \text{Perceived Criminal Injustice} = & a + b_1 \text{ Race} + b_2 \text{ Workers} \\ & + b_3 \text{ Employers} + b_4 \text{ Edu-} \\ & \text{cation} + b_5 \text{ Income} + b_6 \quad (3) \\ & \text{South} + b_7 \text{ Age} + b_8 \text{ Sex} \\ & + b_9 \text{ Central City.} \end{aligned}$$

This set of equations is estimated for each of the 10 perceptions of criminal injustice variables, and then for the scale, using the sample of 1,049 persons with an employed head of household. This preliminary part of our analysis parallels the recent quantitative Marxian studies of income determination (Wright and Perrone 1977; Wright 1978) in its use of a three-class model. We are particularly concerned in equations (1) and (2) with determining separately how being black and being a worker may influence the perception of criminal injustice; in other words, in how being positioned in these separate locations in the social structure may influence perceptions of criminal injustice. However, as noted above, many conflict theorists characterize positions within a social structure along dimensions other than those of race and class. Equation (3) takes into account the race and class distributions of these other positions within the social structure as well as the distribution of race and class within one another. Thus, for example, if blacks are found in equation (1) to perceive significantly more criminal injustice than whites, and if this in fact is due to the greater concentration of blacks in the working class and/or the greater education of whites that puts them into a dominant status culture that assumes equality before the law, then, when class position and education are included in equation (3), the difference in perceived injustice between the races should be reduced substantially or eliminated. Equation (3) actually holds constant the full range of status positions identified above that have a plausible claim to being important determinants of relationships among race, class, and perceived criminal injustice. To the extent that race and class coefficients found in equations (1) and (2) persist in equation (3), with all these structural positions taken into account, we can argue with increased confidence that these coefficients are direct, unmediated consequences of the race-class positions themselves.

Following consideration of the three-class sample, we turn to an analysis of the four-class model that now includes the surplus population of unemployed workers. The analysis outlined above is repeated in this larger sample, with the exception that equations (2a) and (3a) (see table 5 below) include within them the surplus population instead of workers as the b_1 and b_2 coefficients, respectively.

Up to this point, we have considered only an additive, linear model of the perception of criminal injustice. However, this may tell us only part of the story. The last part of our analysis examines the interaction of class with the other status positions by estimating two regression equations within each of the four class categories. This part of the analysis is built around equation (1) and a new equation (within class categories):

$$\begin{aligned} \text{Perceived Criminal Injustice} = & a + b_1 \text{ Race} + b_2 \text{ Education} \\ & + b_3 \text{ Income} + b_4 \text{ South} + b_5 \\ & \text{Age} + b_6 \text{ Sex} + b_7 \text{ Central} \\ & \text{City.} \end{aligned} \quad (4)$$

The latter equation allows examination of the class-race interactions, with other status positions held constant. We are particularly interested here in how the influence of race may be conditioned by class position. For example, there is some reason to expect that the influence of race may be larger in some class categories than others (Wright 1978). The final part of our analysis explores this possibility.

RESULTS

Table 4 presents the results of estimating our regression equations in the three-class sample with the 10 measures of the perception of criminal injustice and the combined scale. The salient finding in this table is clearly the persistent and often striking influence of race on the perception of criminal injustice. In equation (1), with the other status positions of the respondents uncontrolled, the relationship of race to the perception of criminal injustice varies from a low β of .16 to a high of .28, all significant at the .001 level. These perceptions of injustice are largest for those items involving law enforcement officials/police (PCI1, PCI2) and for the item involving courts not treating blacks and other minorities the same as whites (PCI9). Using the 100-point scale as a summary measure and turning our attention to the unstandardized coefficients, we can note that on average, with other class and status positions held constant, blacks score nearly 13 points higher ($b = 12.98$) than whites in their perception of criminal injustice.

TABLE 4

REGRESSIONS OF CRIMINAL JUSTICE ITEMS ON INDEPENDENT VARIABLES IN THREE EQUATIONS FOR THREE-CLASS SAMPLE ($N = 1,049$)

	PCI1	PCI2	PCI3	PCI4	PCI5	PCI6	PCI7	PCI8	PCI9	PCI10	PCI Scale
Eq. (1):											
Race:											
b.....	1.75***	1.64***	1.50***	1.16***	1.36***	1.07***	1.12***	1.40***	2.14***	.94***	14.08***
β25 (.21)	.26 (.19)	.22 (.21)	.17 (.21)	.21 (.20)	.16 (.20)	.17 (.20)	.19 (.23)	.28 (.23)	.14 (.21)	.28 (1.49)
Constant	6.28	5.15	4.69	5.18	5.04	6.45	5.42	6.09	5.52	6.37	56.19
Eq. (2):											
Workers:											
b.....	.25*	-.10	.20	-.02	.27*	.33**	.11	.18	.17	.08	1.45
β06 (.12)	-.03 (.11)	.05 (.13)	-.01 (.12)	.07 (.12)	.08 (.12)	.03 (.12)	.04 (.14)	.04 (.14)	.02 (.13)	.05 (.92)
Constant	6.28	5.36	4.69	5.29	4.99	6.34	5.45	6.11	5.60	6.41	56.54
Eq. (3):											
Race:											
b.....	1.62***	1.57***	1.35***	1.15***	1.16***	.91***	1.05***	1.28***	2.04***	.86***	12.98***
β24 (.21)	.25 (.19)	.20 (.21)	.17 (.21)	.18 (.20)	.14 (.21)	.16 (.21)	.16 (.24)	.27 (.23)	.13 (.22)	.26 (1.53)
Workers:											
b.....	.28	-.07	.02	.10	.07	.35*	.09	.17	.04	.11	1.16
β07 (.15)	-.02 (.13)	.01 (.15)	.03 (.15)	.02 (.14)	.09 (.14)	.02 (.14)	.04 (.16)	.01 (.16)	.03 (.15)	.04 (1.07)

Note.—Numbers in parentheses are SEs.

* Significant at the .05 level.

** Significant at the .01 level.

*** Significant at the .001 level.

TABLE 4 (Continued)

	PCI1	PCI2	PCI3	PCI4	PCI5	PCI6	PCI7	PCI8	PCI9	PCI10	PCI Scale
Employers:											
b.....	.47*	.08	.16	.01	-.12	.32	.23	.22	-.23	.23	1.35
β07 (.23)	.01 (.21)	.02 (.24)	.01 (.23)	-.02 (.22)	.05 (.22)	.03 (.23)	.03 (.26)	-.03 (.26)	.03 (.24)	.03 (1.70)
Education:											
b.....	-.04	-.15**	.09	-.21***	.03	-.07	-.02	.05	-.05	-.03	-.49
β	-.02 (.06)	-.10 (.05)	.06 (.06)	-.13 (.06)	.02 (.05)	-.05 (.05)	-.01 (.05)	-.03 (.06)	-.03 (.06)	-.02 (.06)	-.04 (.40)
Income:											
b.....	-.01	.02	.03	.01	.03	.04	.02	.01	-.02	-.01	.12
β	-.01 (.04)	.02 (.04)	.02 (.04)	.01 (.04)	.03 (.04)	.04 (.04)	.02 (.04)	.01 (.04)	-.01 (.04)	-.01 (.04)	.01 (.29)
South:											
b.....	.26*	.05	-.15	-.04	-.05	.25*	-.20	.33*	.14	.17	.74
β06 (.12)	.01 (.11)	-.04 (.13)	-.01 (.12)	-.02 (.12)	.06 (.12)	-.05 (.12)	.07 (.14)	.03 (.14)	.04 (.13)	.03 (.96)
Age:											
b.....	.04	.03	.01	.05*	.03	-.01	.02	.03	.10***	.01	.31
β05 (.02)	.05 (.02)	.01 (.12)	.07 (.02)	.05 (.02)	-.01 (.02)	.12 (.02)	.04 (.03)	.11 (.03)	.01 (.02)	.06 (.17)
Sex:											
b.....	.08	.11	.13	.30*	.20	-.24*	.14	.12	.36**	.20	1.40
β02 (.12)	.03 (.11)	.03 (.12)	.08 (.12)	.05 (.11)	-.06 (.12)	.04 (.11)	.03 (.13)	.08 (.13)	.05 (.12)	.05 (.85)
Central city:											
b.....	.22	.40***	.47***	.33**	.56***	.20	.40***	.22	.31*	.19	3.32***
β06 (.12)	.11 (.11)	.11 (.12)	.08 (.12)	.15 (.12)	.05 (.12)	.11 (.12)	.05 (.14)	.07 (.14)	-.05 (.13)	.11 (.89)
Constant....	5.75	5.16	4.00	5.19	4.25	6.26	5.08	5.66	4.77	6.15	52.26

NOTE.—Numbers in parentheses are SEs.

* Significant at the .05 level.

** Significant at the .01 level.

*** Significant at the .001 level.

In contrast, there is little evidence in table 4 that workers perceive more criminal injustice than members of other classes or that employers perceive less. For three of the 10 dependent measures for which equation (1) is estimated, workers perceive significantly more injustice than others; however, all of these relationships are below .10 in magnitude, and only one, that involving the treatment the poor receive from lawyers (PCI6), remains significant when the other status positions of the respondents are held constant in equation (3). The worker coefficients for the summary scale are both small and nonsignificant.

Of the remaining measures of status position, residence in a central city has the most interesting influence. For six of the 10 dependent measures in equation (3), residing in a central city significantly increases the perception of criminal injustice, even when all other status measures, including race and class, are taken into account. In terms of the summary scale, central city residents score more than three points ($b = 3.32$) higher than residents of other areas. The remaining status positions show only scattered influence in table 4.

Table 5 presents the results of reestimating a modified set of the regression equations above for the four-class sample that now includes the surplus population; this surplus population replaces workers in two of the equations ([2a] and [3a]). The most striking difference between the findings reported in tables 5 and 4 is the influence of the added class: for nine of the 10 dependent measures and the summary scale, members of the surplus population are significantly more likely than others to perceive criminal injustice. The influence of this class position on the perceptual measures is not as large or as varied as the influence of race; however, it does withstand controls for all other status positions in equation (3a). Using unstandardized coefficients and the summary scale as our point of reference, before controlling for their other status positions, we find that members of the surplus population on average score approximately nine points higher than others in their perceptions of criminal injustice; and, after we control for status positions, members of the surplus population on average score nearly seven points ($b = 6.90$) higher. Use of a four-class model, then, is crucial to the class-based concerns of this paper. Elsewhere, the results of changing from a three- to a four-class model are slight. Central city residential status continues to have a notable influence, and race continues to have a striking influence; both withstand controls for other status positions in equation (3a).

Part A of table 6 presents the results of estimating regression equations (1) and (4) for the summary scale within each of the four classes. Most significantly, this table reveals that the importance of these class categories extends beyond what our preceding analysis suggests. Our focus in

this part of the analysis is on the influence of race within each of the classes, or, in other words, on race-class interactions. As one might expect, there are few blacks in the employer class, and only two blacks of this class are in our sample. Thus race could not be expected to have a statistically significant impact on employers' perceptions of criminal injustice, and we have therefore omitted this variable from the equations estimated in this class category. However, what interests us most is the influence of race in each of the other three classes: this influence decreases with each step down the class structure. Looking first at equation (1) in the professional-managerial class, we see that blacks on average score 20 points higher ($b = 20.09$, $\beta = .34$) than whites in their perceptions of criminal injustice. In equation (4), with other status positions held constant, this difference slips only slightly to 18.45 points. On the other hand, black workers score an average of about 12 points ($b = 12.12$, $\beta = .26$) higher than white workers on this scale in equation (1), and this difference again drops only slightly to 11.30 in equation (4). Finally, black members of the surplus population on average score only about five points higher ($b = 5.18$, $\beta = .14$) than white members of this class in equation (1), and, with other status positions taken into account, this difference drops to about three points ($b = 3.54$; $\beta = .10$) in equation (4).

While the race coefficients in equations (1) and (4) are both highly significant in the PMC and among workers, this is not the case in the surplus population. However, the small number of persons in the surplus population ($N = 59$) limits the importance that can be attached to the latter finding. Much more meaningful are the t -values presented in part B of table 6 for the three possible comparisons of class category race coefficients.⁵ All six of these t -values are statistically significant at the .001 level, indicating that comparisons of race coefficients between the surplus population and workers, between the surplus population and the PMC, and between workers and the PMC all involve differences that are highly unlikely to be products of chance.

An intuitive way of clarifying the findings above is to calculate predicted values using equation (4) for each of the race-class combinations discussed. To do this it is necessary to establish at what values of the independent variables in equation (4) we wish to make our comparisons. One approach to this problem used by Duncan (1969) is to substitute the

⁵ Our paired comparisons of the race slopes for the different groups are based on the following t -test:

$$t = (b_{11} - b_{12}) / \sqrt{(\nu_1 S^2 b_{11} + \nu_2 S^2 b_{12}) / (\nu_1 + \nu_2)},$$

where b_{11} is the race coefficient for group 1; b_{12} is the race coefficient for group 2; $S^2 b_{11}$ and $S^2 b_{12}$ are the standard errors of the coefficients for groups 1 and 2, respectively; and ν_1 and ν_2 are the degrees of freedom for groups 1 and 2, respectively.

TABLE 5

REGRESSIONS OF CRIMINAL JUSTICE ITEMS ON INDEPENDENT VARIABLES IN THREE EQUATIONS FOR FOUR-CLASS SAMPLE ($N = 1,106$)

	PCI1	PCI2	PCI3	PCI4	PCI5	PCI6	PCI7	PCI8	PCI9	PCI10	PCI Scale
Eq. (1):											
Race:											
b.....	1.67***	1.62***	1.54***	1.15***	1.37***	1.03***	1.13***	1.31***	2.05***	.86***	13.71***
β25 (.19)	.26 (.18)	.23 (.20)	.17 (.20)	.22 (.19)	.16 (.19)	.18 (.19)	.18 (.21)	.27 (.21)	.13 (.20)	.28 (1.42)
Constant	6.31	5.18	4.74	5.19	5.08	6.50	5.46	6.14	5.58	6.41	56.60
Eq. (2a):											
Surplus											
popu- lation:											
b.....	.77**	.79**	1.27***	.36	1.04***	.89***	.96***	.95***	1.33***	.76**	9.14***
β09 (.26)	.10 (.24)	.14 (.27)	.04 (.26)	.12 (.25)	.11 (.25)	.11 (.25)	.10 (.28)	.13 (.29)	.09 (.26)	.14 (1.93)
Constant	5.66	4.51	3.54	4.91	4.12	5.66	4.56	5.27	4.37	5.69	48.31
Eq. (3a):											
Race:											
b.....	1.52***	1.53***	1.32***	1.13***	1.10***	.84***	1.02***	1.12***	1.89***	.74***	12.23***
β23 (.20)	.25 (.19)	.19 (.20)	.17 (.20)	.17 (.19)	.13 (.20)	.16 (.20)	.16 (.22)	.25 (.22)	.11 (.21)	.25 (1.47)
Surplus											
popu- lation:											
b.....	.53*	.58*	.98***	.20	.70**	.61*	.77**	.75*	1.05***	.71**	6.90***
β06 (.27)	.07 (.25)	.11 (.28)	.02 (.27)	.08 (.26)	.07 (.27)	.09 (.25)	.08 (.29)	.11 (.30)	.08 (.27)	.11 (1.96)

NOTE.—Numbers in parentheses are SEs.

* Significant at .05 level.

** Significant at .01 level.

*** Significant at .001 level.

TABLE 5 (Continued)

	PCI1	PCI2	PCI3	PCI4	PCI5	PCI6	PCI7	PCI8	PCI9	PCI10	PCI Scale
Employers:											
b.....	.30	.12	.14	-.03	-.16	.12	.18	.11	-.27	.15	.65
β04 (.22)	.02 (.20)	.02 (.23)	-.01 (.22)	-.02 (.21)	.02 (.22)	.03 (.21)	.01 (.24)	-.03 (.24)	.02 (.22)	.01 (1.60)
Education:											
b.....	.01	-.13**	.08	-.17**	.04	-.01	.01	-.02	-.04	-.02	-.27
β01 (.05)	-.09 (.05)	.15 (.05)	-.11 (.05)	.03 (.05)	-.01 (.05)	.01 (.05)	-.01 (.06)	-.03 (.06)	-.02 (.05)	-.02 (.37)
Income:											
b.....	.01	.01	.03	.02	.05	-.06	.01	.01	-.02	-.02	.18
β01 (.04)	.01 (.03)	.03 (.04)	.02 (.04)	.05 (.04)	.05 (.04)	.01 (.04)	.01 (.04)	-.02 (.04)	-.02 (.05)	.02 (.27)
South:											
b.....	.26*	.07	-.15	-.04	-.07	.21	-.20	.33*	.15	.19	.75
β06 (.12)	.02 (.11)	-.03 (.12)	-.01 (.12)	-.02 (.12)	.05 (.12)	-.05 (.12)	.07 (.13)	.03 (.13)	.05 (.12)	.03 (.88)
Age:											
b.....	.04	.04	.01	.05*	.03	.01	.02	.04	.10***	.01	.33
β05 (.02)	.05 (.02)	.01 (.02)	.07 (.02)	.04 (.02)	.01 (.02)	.03 (.02)	.04 (.03)	.11 (.03)	.01 (.02)	.06 (.17)
Sex:											
b.....	.08	.11	.14	.27*	.20	-.27*	.11	.09	.35**	.18	1.27
β02 (.11)	.03 (.11)	.03 (.12)	.07 (.12)	.05 (.11)	-.07 (.11)	.03 (.11)	.02 (.13)	.08 (.13)	.05 (.12)	.04 (.83)
Central city:											
b.....	.21	.34**	.41***	.31*	.54***	.13	.35**	.19	.25	.16	2.87***
β05 (.12)	.09 (.11)	.10 (.12)	.08 (.12)	.14 (.11)	.03 (.12)	.09 (.12)	.04 (.13)	.05 (.13)	.03 (.12)	.10 (.87)
Constant....	5.21	4.50	3.10	4.85	3.57	5.60	4.31	4.89	3.75	5.53	45.32

NOTE.—Numbers in parentheses are SEs.

* Significant at .05 level.

** Significant at .01 level.

*** Significant at .001 level.

most advantaged group's means into the other groups' equations. This approach tells us how the other groups would do if they had the same mean values as the most advantaged group on the independent variables considered. A second approach, used by Wright (1978), is to sum the means

TABLE 6
COMPARISONS WITHIN CLASS CATEGORIES
A. REGRESSIONS OF CRIMINAL JUSTICE SCALE ON INDEPENDENT
VARIABLES IN TWO EQUATIONS WITHIN CLASS CATEGORIES

	Employers*	PMC	Workers	Surplus Population
Eq. (1):				
Race:				
b.....	...	20.09**	12.12**	5.18
β34 (3.18)	.26 (1.74)	.14 (4.71)
Constant.....	...	55.20	56.67	65.43
Eq. (4):				
Race:				
b.....	...	18.45**	11.30**	3.54
β31 (3.19)	.25 (1.81)	.10 (5.74)
Education:				
b.....	2.50*	-.65	-.63	1.48
β27 (1.15)	-.05 (.70)	-.05 (.55)	.13 (1.82)
Income:				
b.....	-.53	-.41	.47	.16
β	-.07 (.93)	-.04 (.53)	.05 (.37)	.02 (1.29)
South:				
b.....	2.47	-.04	.63	.33
β10 (2.80)	-.01 (1.71)	.02 (1.14)	.01 (4.44)
Age:				
b.....	1.09	.05	.30	.01
β21 (.57)	.01 (.33)	.05 (.22)	.01 (.93)
Sex:				
b.....	-5.53*	2.30	1.50	.46
β	-.23 (2.74)	.08 (1.61)	.05 (1.08)	.02 (4.30)
Central cities:				
b.....	-1.53	5.89**	2.50*	-5.42
β	-.06 (2.95)	.19 (1.68)	.09 (1.12)	-.18 (4.13)
Constant.....	45.99	54.74	52.88	60.87

B. COMPARISONS OF CLASS CATEGORY RACE COEFFICIENTS

	SURPLUS POPULATION COMPARED WITH WORKERS		SURPLUS POPULATION COMPARED WITH PMC		WORKERS COMPARED WITH PMC	
	Eq. (1)	Eq. (4)	Eq. (1)	Eq. (4)	Eq. (1)	Eq. (4)
Difference in race coefficients.....	6.94	7.76	14.91	14.91	7.97	7.15
t-value of difference	4.92**	5.35**	7.72**	7.89**	5.39**	4.77**
Surplus population (%)...	.43	.31	.26	.19

* Race is omitted from eq. (4) in this class category; see text for explanation.

* Significant at .05 level.

** Significant at .001 level.

of the groups involved and then divide by the number of groups. This approach attempts to establish a middle ground as the point of comparison. With our data, both approaches yield very similar results. The following results come from using Duncan's approach and substituting the white employers' means into each of the six equations involved: in the surplus population, whites score an average of 64.50 on the criminal injustice scale, while blacks score an average of 68.13; in the working class, whites score an average of 55.80, while blacks score an average of 67.10; and in the PMC, whites score an average of 54.57, while blacks score an average of 73.02. These scores suggest that in the surplus population there is a consciousness of class position vis-à-vis the criminal justice system that largely overcomes factors of race. However, as we move up the class structure to the working class and into the PMC, blacks and whites are increasingly divided in their perceptions of criminal injustice. This occurs in the following way. On the one hand, whites tend to moderate their views as they move up the social structure: white workers perceive less criminal injustice than white members of the surplus population, and white members of the PMC are slightly less likely still to perceive criminal injustice. On the other hand, blacks in the working class are much like blacks in the surplus population in their levels of perceived criminal injustice, and black members of the PMC are the most likely persons in the sample to perceive criminal injustice. Put back into more abstract summary terms, class position conditions the influence of race in the perception of criminal injustice. We will note a significant analogue to this finding in the conclusion of this paper.

Two other findings in table 6 are worthy of note, although we will not dwell on them here. The first is that there are signs that central city residential status is conditioned by class position in a manner similar to race. In particular, members of the PMC show signs of perceiving more criminal injustice when they reside in central cities ($b = 5.89$, $P < .001$). This finding does not contradict the work of Frank, and it is fully consistent with the Ehrenreichs' focus on radical movements within the PMC. The second finding is that among employers in table 6, it is highly educated men who are least likely, and less educated women who are most likely, to perceive criminal injustice. We will only note here that, following Collins, it is tempting to interpret the more highly educated male view as an expression of successful indoctrination into elite culture.

CONCLUSIONS

Two prominent research literatures, one dealing with criminal sentencing and the other with public rankings of the seriousness of crimes, have failed

to find evidence supportive of a conflict theory of criminal justice. We have addressed issues raised by this theory in a different and more direct way: we have examined divisions, particularly those of race and class, in public perceptions of criminal injustice. In this paper we have presented a scale of perceived criminal injustice that shows every sign of being highly reliable and valid. Our analysis of a national survey including this scale and its component parts produced three major findings: (1) that black Americans are considerably more likely than white Americans to perceive criminal injustice; (2) that regardless of race, members of the surplus population are significantly more likely than members of other classes to perceive criminal injustice; and (3) that class position conditions the relationship of race to the perception of criminal injustice, with the division between the races in these perceptions being most acute in the professional managerial class. In contrast to the prior research literatures reviewed in this paper, then, we have found substantial evidence that race and class conflict exist with regard to issues of criminal injustice in America. In addition, we found evidence that the perception of criminal injustice is more pronounced in the metropolitan centers than in peripheral parts of this nation.

The third finding above, involving the race-class interaction and the observation that the races are most divided in the PMC, deserves separate comment. This finding parallels in an interesting way the finding of Wright (1978) that blacks also receive the smallest returns in income for their education in the managerial class: that is, that income discrimination by race may be largest in this class. The latter finding increases our confidence in the former and encourages several alternative explanations of it. One possibility is that black members of the PMC who perceive income discrimination may be sensitive to the perception of injustice elsewhere as well. Another possibility is that a consciousness of race among blacks in the PMC, and their more recent movement into this class, is associated with a consciousness of prior class position that continues to influence significantly their perceptions of the surrounding world. Finally, it may be that whites who manage to stay out of the surplus population also are able to avoid criminal injustice, while working class blacks and black members of the PMC may continue, directly or indirectly, to experience criminal injustice, perhaps partly as a consequence of the apparent contradiction in their race and class positions. Our data do not allow us to do more than suggest these possibilities and their suitability as subjects for further research.

The findings we have reported are important for Marxists and non-Marxists alike. For Marxists they are a vindication of the point that class position is indeed significant to the understanding of crime and criminal-

justice-related conflicts. In particular, Marxian criminologists who have emphasized that consideration of the surplus population is especially important in understanding such conflicts receive support from these data and, beyond this, encouragement with regard to this class being and/or becoming a "class for itself" in terms of a common consciousness of its position in relation to the criminal justice system. Non-Marxists, some of whom may be reluctant to concede that class injustice actually exists in America, will nonetheless likely agree that *perceptions* of this injustice do exist, and they can now join in the important task of explaining these perceptions. In doing so they will undoubtedly note that the influence of class is apparently smaller than that of race in explaining the perception of criminal injustice; and they could reasonably argue from this that while Marxian *theory* finds some support in these kinds of data, nonetheless, the base of class consciousness for bringing about the revolutionary events that Marx predicted, and Marxian criminologists foresee, is small. There is much room here for research and debate. Our point is simply that Marxists and non-Marxists alike will benefit from operationalizing the concept of class, and issues of criminal injustice, in ways that parallel those suggested in this paper. Indeed, our data indicate that any model of the perception of criminal injustice that does not include measures of race and class similar to these is likely to be incompletely specified.

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Do Denominations Matter?¹

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This study focuses on the significance of denominational differentiation for American religious life. Some scholars have argued that the affiliates of the various Protestant denominations are becoming more and more alike in their socioeconomic and demographic characteristics and in religious behavior and orientations. Hence, they argue, denominational affiliation has little independent impact on either religious or secular behavior. Others have suggested that denominational affiliations continue to be important influences on individual behavior. In this paper we analyze data from a national survey of American Jews which show that denominational differences are substantial and more influential within this highly educated and acculturated minority than they have commonly been assumed to be. These findings suggest that recent research may have underestimated the potential importance of contemporary denominational differentiation for American religious life.

For centuries, American society has been distinguished by its vast array of religious and spiritual groups (Gaustad 1962; Ahlstrom 1972). The 1970s witnessed an upsurge in new and unconventional religious groups (Glock and Bellah 1976) as well as a retrenchment among many of the "main-line," institutionalized religious bodies (Carroll, Johnson, and Marty 1977; Hoge and Roozen 1979; Kelly 1972; Marty 1976). In response to these trends, there has been a renewal of interest in understanding and characterizing the situation of established religious bodies in America. We wish to contribute to this renewed study of institutionalized religion by

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examining denominationalism—a central element in the development of American Protestantism and an important influence on Catholicism and Judaism in America.

Wilson (1968b, p. 434) has provided us with one of the most useful characterizations of the denominational form:

Denominationalism is the typical form of religious organization in the pluralist society. The denomination does not claim universal allegiance co-terminous with the state as does the church; it claims less institutional objectivity. Like a sect, it emphasizes voluntary allegiance, but it claims no monopoly on the truth and demands much less of its adherents in the way of doctrine and morals. As an institutional form, it evidences the diminution and compartmentalization of religious influence in industrial society. Even where it inherits the hierarchic authority structure of the church, it offers more lay opportunity in government. Its religious leadership, in contrast with that of most sects, tends to become bureaucratized.

As Wilson's comments suggest, denominationalism implies both an organizational form and an ideological stance that accepts, and even legitimates, the coexistence of diverse bodies differing in theology, ritual, and government, but drawing on the same religious tradition (Parsons 1960, p. 295; Mead 1963).

Although chiefly a Protestant phenomenon, denominationalism has also had an impact on non-Protestant religious groups. American Catholicism retained its unity but adjusted itself organizationally and ideologically to the voluntarism of American life. In response to Protestant and secular influences, it shifted toward greater involvement by lay people and more cooperation with non-Catholic religious groups (Koller 1979; Greeley 1972, pp. 186, 194). In American Judaism the process of "Protestantization" (Blau 1976) was more far-reaching. Gradually, a tripartite structure of national religious organizations emerged that resembles Protestant denominationalism in its representation of various shades of belief and practice within distinct denominational bodies and in the social differentiation of the adherents of these bodies. The tacit acknowledgment by the elites of each of the three major Jewish groups that the other groups also have a legitimate place within the American Jewish scene and the development of cooperative ties to non-Jewish groups indicate considerable acceptance of the principle of religious pluralism. Among Jewish lay people who identify with Reform and Conservative Judaism, this pluralistic stance is nearly universal.

CURRENT INTERPRETATIONS OF DENOMINATIONALISM

Despite the emphasis of denominational leaders on their distinctive interpretations of doctrine and practice, sociologists have usually followed

Niebuhr's (1929) lead in stressing the "social sources of denominationalism." Thus, the emergence and persistence of a vast array of religious bodies within the Protestant tradition has been seen as reflecting differences of class, race, culture, region, and ethnicity (Roof 1979; Demerath 1965; Laumann 1969). Conversely, the wave of denominational mergers which began in the 1960s and the growth of interdenominational cooperation are often interpreted as reflecting the reduction of those social differences which had previously divided the denominations (e.g., Lee 1960; Herberg 1960). It has also been suggested that, with the gradual erosion of religious traditionalism and the growth of secularity (Wuthnow 1976; Hoge and Roozen 1979), those religious issues that previously divided the various moderate and liberal denominations have been forgotten. These trends, combined with declines in membership among the main-line denominations (Hoge and Roozen 1979), have apparently led the elites within established religious bodies to find common cause in attempting to promote their interests in the face of secularization (Berger 1969; Wilson 1968a).

Additional light on the social sources of denominationalism comes from studies of interdenominational mobility. People are especially likely to switch their denominational affiliation when they undergo geographic or socioeconomic mobility, when they marry persons with different denominational backgrounds, and when they become independent of their parents and discard the affiliations of their youth (Greeley 1970; Newport 1979). The regional character and social status of denominations play an important part in their selection (Klugel [1979] and Mueller [1971] on region; Stark and Glock [1968], Mueller [1971], and Newport [1979] on status). Liturgical differences and a division between liberal and conservative denominations also shape denominational mobility patterns, while finer doctrinal distinctions have little impact.

The growth of movements which cut across denominational lines and sometimes even divide members of the same church raises further questions about the relevance of denominational affiliation for individual beliefs and behavior. During the past two decades, adherents to Protestantism's tradition of liberalism and social action have focused their churches' concern on such causes as civil rights, opposition to U.S. involvement in Vietnam, and equality of the sexes. In contrast, religiously (and sometimes politically) conservative forces within Protestantism have achieved a renewed vigor and prominence through the evangelical and charismatic renewals occurring in main-line denominations (Quebedeaux 1976, 1978; Marty 1976). In the light of such developments, it is not surprising that studies have found a wide range of religious orientations among members of the same denomination (e.g., Hoge 1976; Ethridge and Feagin 1979).

These religious developments might well lead us to ask whether current

patterns of denominational affiliation are anything more than artifacts of long-forgotten historical developments. At the individual level of analysis, we may legitimately inquire whether denominational preference and affiliation have significant influences on people's religious orientations and behavior.

The survey research which has focused on these issues has produced ambiguous findings. Some have argued that denominational affiliation still has significance for religious orientations although the meaning of such affiliation has been transformed in recent decades. In the mid-1960s, Glock and Stark (1965; also Stark and Glock 1968) found that even though historic differences in belief among denominations had become blurred church members could still be grouped into distinctive ideological camps, ranging from liberal to moderate, conservative, and fundamentalist (see Carroll and Roozen [1977] for a similar view). Others (e.g., McLoughlin 1968; Johnson 1967; Broughton 1975) have confirmed the importance of the doctrinal and behavioral split between the liberal and conservative denominations while eschewing finer distinctions. Further evidence of the importance of this split comes from research documenting substantial declines during the late 1960s and early 1970s in affiliation and participation in the liberal churches, as contrasted to the growth of the conservative churches (Carroll and Roozen 1977; Hoge and Roozen 1979; Kelley 1972). Unfortunately, none of the studies of denominational differences in belief and practice explored the extent to which these differences reflect the socioeconomic and subcultural characteristics of church members.

There is greater controversy regarding the social consequences and significance of denominational affiliation and belief. A few early inquiries (e.g., Mayer and Sharp 1962; Schuman 1971) found differences in the economic orientations of adherents to different denominations. Others documented the existence of denominational differences in political orientations among both clergy and laity (Johnson 1964; Jeffries and Tygart 1974; Hadden 1969). Moreover, people having no religious preference have been found to be more liberal, both socially and politically (Hoge and Roozen 1979, p. 103). Unfortunately, many of these studies have relied on small or biased samples of church members and have not introduced adequate multivariate controls. Viewed as a whole, the literature yields little in the way of unambiguous, consistent evidence on the impact of denominational affiliation on economic, political, or social behavior (see Bouma [1973], Winter [1974], Schuman [1971], and Rojek [1973] for recent discussions).

Hitherto, the only available data that would allow us to examine these questions empirically have pertained to American Protestants. Yet, as we noted earlier, denominationalism is also a basic characteristic of American Jewish life. In addition to being of interest in its own right, the study of

denominationalism within American Jewry can be of considerable value for our understanding of the denominational phenomenon in general. Over the past three decades, American Jewry has become increasingly socio-culturally homogeneous and highly Americanized. Moreover, Jewish Americans today have occupational, educational, and economic standing comparable with the most successful of the older Protestant subgroups (Lazerwitz 1961; Glenn and Hyland 1967; Roof 1979). If denominational differences in religious practice and ideology and in secular behavior can be shown to persist within American Jewry despite these high levels of homogeneity and Americanization, and if such differences can be shown to be independent of the socioeconomic and demographic characteristics of the affiliates of the various denominational groups, then it would seem necessary to investigate more carefully denominational patterns within the more heterogeneous Protestant community.

Jewish Denominational Differentiation

Most studies of contemporary American Judaism have treated denominational differences among the laity as a marginal phenomenon. Some empirical studies (e.g., Goldstein and Goldscheider 1968; Axelrod, Fowler, and Gurin 1967) treat denominational preference as an indicator of the strength of Jewish identification and show that, with Americanization, preference for the more tradition-oriented Orthodox and Conservative groups declines while preference for the Reform group and nonaffiliation increases. Others (Sklare 1971; Sklare and Greenblum 1967) deemphasize the basic denominational divisions and, instead, stress the wide variation among synagogues of the same denomination. Elazar (1967) argues that as a result of the dominance of voluntarism and congregationalism in American Judaism, denominations must tolerate a wide range of behavior and orientations among their local synagogues. Synagogues, in turn, must tolerate wide variations among their congregants.

Despite general agreement about these basic tendencies in American Jewish life, there are some indications that denominational differences will continue to be important within American Jewry and may even sharpen. Although cooperation has grown within American Jewry among religious groups and between religious and nonreligious organizations, there is also a countertrend. Some denominational leaders, particularly among the Orthodox, articulate and even sharpen their distinctive denominational positions (see Liebman 1965; Glazer 1972). Moreover, the growth of Jewish parochial schools organized along denominational lines may well heighten the impact of denominational leaders. To the extent that such elite orientations become dominant, denominational differences will increase.

Despite the occasional discussion of such countertrends, there seems to be a developing consensus among many students of American Jewish life that denominational differences are primarily the result of variations in the degree of Americanization and socioeconomic mobility of the members of denominations. Ideological differences are assumed to be meaningful only for the leaders and the lay elites directly influenced by them. Moreover, differences in ideology and religious observance which have persisted are often assumed to be irrelevant to secular behavior.

Yet such interpretations of American Jewish denominational patterns have been made without the help of systematic nationwide data. The National Jewish Population Survey (Lazerwitz 1973*b*, 1974) has made such national data available for the first time. Analysis of these data can help provide a more nearly definitive understanding of the degree to which denominational identification and affiliation are themselves independent determinants of individual religious and secular behavior. Elsewhere we have analyzed these data to create a basic social and religious profile of the adherents to the major Jewish denominational groupings (Lazerwitz and Harrison 1979). In that report we showed that social and cultural differences among the adherents to the various denominational groupings continue to exist, although they are apparently less pronounced than in the past. Moreover, we showed that there are still considerable ideological and behavioral differences among adherents of the various denominations.

The Research Problem

Here we wish to examine two questions to which we alluded in that earlier report, both of which can be answered only through multivariate analysis. First, to what extent are the differences in the ethnic and religious orientations of those identifying or affiliating with the denominations a function of their individual socioeconomic and demographic characteristics? Second, are there any independent effects of denominational affiliation and identification on secular behavior once controls have been introduced for the individual characteristics of the adherents? Our answers to these questions show that denominational differences among American Jews are much more substantial, durable, and influential than has been assumed previously.

METHODS

Sample Design

The National Jewish Population Survey (NJPS) was a national survey of the United States Jewish population conducted from the early spring of

1970 to the end of 1971 for the Council of Jewish Federations and Welfare Funds. The sample design took into account the fact that American Jews constitute only a small percentage of the total American population, that a sizable number do not live in neighborhoods with high concentrations of Jewish residents, and that many are not listed on readily available communal lists. The final design was a complex, multistage, two-phase, disproportionately stratified cluster sample. This design was guided by a variation of the city directory-block supplement sampling approach described in Lazerwitz (1968, pp. 314-20) and Kish (1965, pp. 352-58). It permitted combining lists and area samples so that households came in through only one source and no meaningful design bias was encountered in finding scattered Jewish households. To save space, we will not repeat details of the design, response characteristics, or generalized sampling errors that appear in NJPS reports (see Lazerwitz 1973*b*, 1974, 1978).

When a sampled household was found to have at least one Jewish resident, basic family information was obtained. This resulted in 5,790 household interviews at a 79% response rate. Then, via the Kish (1949) technique, one adult Jewish respondent was selected from among all the Jewish adults in residence for additional questions. At this point, there was an additional subsample in New York City. Hence, full interviews were obtained from 4,305 Jewish adults.

Major Variables

For reference, we note here the main characteristics of the variables used in this analysis.

1. *Jewish denominational identification and synagogue membership.*—Respondents were classified into four categories on the basis of their expressed denominational identifications. Individuals who did not identify with a denomination or said they were "just Jewish" were classified as having no denominational identification. In addition, respondents were asked whether they were members of a synagogue. In order to use both of these variables at once, we cross tabulated the four-category denominational preference variable and the dichotomous membership variable. This cross-tabulation yielded an eightfold typology of denominational orientations. However, the sample sizes of two of the eight types were too small to justify their inclusion in statistical analyses. First, respondents without a denominational identification rarely reported synagogue membership. Second, only 174 respondents who identified themselves as Orthodox Jews were not members of a synagogue. Therefore, in the analysis which follows, we shall report on just the six major combinations of denominational identification and affiliation.

2. *Jewish identification indices.*—A set of items indicative of various

aspects of religious and ethnic identification was used to create indices of nine dimensions of Jewish identification. These indices are described in several previous reports (Lazerwitz 1973a, 1978; Lazerwitz and Harrison 1979). Here, the Appendix provides a more detailed description of these and other relevant variables. The nine dimensions measured are childhood Jewish background, Jewish education, religious observance, pietism, Jewish ideology, primary group involvement, Jewish organizational involvement, Jewish socialization of one's children, and concern for world Jewry.

3. *Secular correlates of Jewish identification.*—In addition, we created indices of involvement in general (non-Jewish) voluntary associations and political liberalism.

4. *Socioeconomic and demographic characteristics.*—Variables measured included secular education, occupation of the head of the household, yearly family income, sex, age, generation in the United States, and stage in the family life cycle (see Appendix for more details).

THE FINDINGS

The Extent of Denominational Differences in Ethnoreligious Orientations and in Social and Cultural Characteristics

We may profitably use a noninterval, multivariate approach to investigate the extent to which the members of the six denominational subgroups are differentiated in their ethnoreligious orientations and their social and cultural characteristics. Multivariate nominal scale analysis (MNA) is a version of dummy multiple regression that does not require the assumption of linearity. Its variables can be either all ordinal or all nominal.²

Through MNA it is possible to examine the extent to which individuals having different denominational identifications and memberships are clearly differentiated on the set of ethnic, religious, and social variables described above. Thereafter, it will be possible to examine which of the variables differentiate most sharply among the denominational groups.³

² This approach assumes no interaction among its variables. This assumption was checked by the technique called THAID, and no meaningful interactions were found. These methods, which are part of the Osiris computer program package, are described in Andrews and Messinger (1973) for MNA and in Morgan and Messinger (1973) for THAID.

³ A word about significance testing is in order. First, the complex, multistage, clustered sample requires special sampling error treatment. Such treatment is presented, in detail, in Lazerwitz (1974). In general, the sample design effect, the blow-up over simple random sampling, is 3.5. Anyone who wants to do significance testing with any of the percentages of this article would have to multiply simple random sampling errors by 3.5 before plugging them into any significance testing equation. The estimation of sampling errors for MNA components is a still more complex matter—so complex, in fact, that it is seldom done. Hence, for this article, we have relied on the presence of consistent data patterns to back up conclusions without computing specific

TABLE 1
PREDICTION OF DENOMINATION USING MNA FOR SYNAGOGUE
MEMBERS AND NONMEMBERS

ACTUAL DENOMINATIONAL PREFERENCE	PREDICTED DENOMINATIONAL PREFERENCE (%)					N
	Orthodox	Conser- vative	Reform	No Preference	Base	
A. Synagogue members:						
Orthodox.....	36	58	6	0	100	398
Conservative.....	5	77	18	0	100	1159
Reform.....	1	33	66	0	100	841
B. Nonmembers:						
Conservative.....	1	76	14	9	100	616
Reform.....	0	23	60	17	100	548
No preference.....	1	10	23	66	100	472

SOURCE.—Data were developed from the original computer tapes of the National Jewish Population Service, 1971.

Table 1 shows the results of an attempt to use all the variables to predict the denominational identifications of synagogue members and nonmembers. Part A of the table shows that for synagogue members we are able to predict correctly the affiliation of only 36% of the Orthodox, but that denominational affiliation is correctly predicted for 77% of the Conservative members and 66% of the Reform members. The latter two denominations encompass the vast majority of synagogue members in the United States. As part B shows, the prediction patterns among adults who are not synagogue members are quite similar to those of synagogue members. Conservative affiliation, Reform affiliation, and the lack of a denominational preference are predicted correctly quite frequently. All told, the preferences of 65% of synagogue members and 64% of nonmembers were correctly predicted by the set of ethnic, religious, socioeconomic, and demographic variables that formed the MNA equations. Thus, it is clear that most of the denominational subgroups, apart from the Orthodox, are readily differentiated from one another in their ethnoreligious orientations, behavior, and social characteristics.⁴

Table 2 gives the MNA beta coefficients for the variables listed above on the six denominational preference-synagogue membership categories.⁵

cluster-sample significance tests for regression components. Kish and Frankel (1970) compute cluster-sampling errors for regression components and show how difficult it is to do even with their quite sophisticated special computer program. In our judgment, the important data patterns are consistent enough for it to be quite unlikely that they result from chance variations.

⁴ Half of the sample adults claimed synagogue membership. Among all Jewish adults (synagogue members or not) 11% preferred the Orthodox denomination, 42% the Conservative, and 33% the Reform, with 14% having no denominational preferences. Hence, the ability to predict the preference of only a minority of Orthodox Jews has little impact on overall predictive percentages in the MNA results.

⁵ These beta coefficients measure the regression associations between each indicated

It shows that the six are only weakly differentiated in terms of the socioeconomic characteristics of their members. In contrast, there are still enduring generational differences among the denominations. The table also shows that there are substantial differences between the groups in religious behavior and beliefs. Moreover, these differences are not simply a function of socioeconomic and demographic characteristics of the groups' adherents. The large beta values for religious behavior and pietism reflect denominational differences *after* controls have been introduced for demographic and socioeconomic characteristics of the respondents. Thus, despite their growing social and cultural similarity, Jewish denominations continue to be sharply differentiated in terms of the beliefs and practices of their members. Elsewhere (Lazerwitz and Harrison 1979) we have shown that the

TABLE 2

MNA BETA VALUES BY JEWISH DENOMINATION FOR DEMOGRAPHIC, SES,
AND JEWISH IDENTIFICATION INDICES AND FOR SECULAR CORRELATES

	SYNAGOGUE MEMBERS			NONMEMBERS		
	Orthodox	Conser- vative	Reform	Conser- vative	Reform	No Preference
Demographic:						
Age.....	.04	.02	.04	.06	.11	.11
Sex.....	.03	.09	.09	.01	.01	.01
Life cycle.....	.14	.20	.17	.11	.12	.09
U.S. generation.....	.13	.10	.16	.12	.17	.20
SES:						
Education.....	.04	.04	.03	.04	.03	.05
Occupation.....	.07	.07	.02	.08	.08	.10
Income.....	.01	.06	.05	.05	.06	.03
Jewish identity:						
Jewish background.....	.10	.07	.07	.05	.08	.08
Jewish education.....	.07	.03	.02	.04	.02	.04
Religious behavior.....	.16	.08	.17	.19	.15	.16
Pietism.....	.08	.13	.18	.20	.08	.24
Ideology.....	.06	.02	.07	.11	.14	.05
Primary group involvement.....	.05	.08	.12	.05	.08	.14
Jewish organizational involvement.....	.09	0	.04	.06	.06	.09
Jewish child socialization	.07	.07	.05	.03	.11	.08
Concern for world Jewry	.07	.06	.02	.06	.03	.02
Secular correlates:						
General organizational involvement.....	.08	.06	.12	.10	.14	.07
Political liberalism.....	.03	.12	.10	.09	.07	.13

SOURCE.—See table 1.

variable and a specific denomination with all the other variables controlled. Project analysis has shown it feasible to adopt the following convention. (a) Weak beta values are those less than .10 or between $-.09$ and 0 . (b) Moderate beta values are those from .10 to .19 or $-.10$ to $-.19$. (c) Strong beta values are those greater than .19 or $-.19$.

denominations may be ranked on most of the indices of religious observance and ethnic identification from the Orthodox, who are the most strongly identified, to the Conservative, the Reform, and those without a denominational preference. Although the differences in these areas may be smaller than they were in the past, they are still very substantial.

Despite the differences between denominations, our data show that there is considerable unity among Jewish Americans in their allegiance to the state of Israel and their concern for the well-being of world Jewry. No meaningful differences occur between the denominational subgroups in these orientations, as reflected in the index of concern for world Jewry.

Secular Consequences of Denominational Identification and Affiliation

Table 2 also allows us to examine the second empirical issue raised above. To what extent do denominational identification and affiliation have an independent impact on secular behavior? Two measures, one of involvement in general (non-Jewish) community voluntary associations and the other of political liberalism, provide some indication of the orientations of the respondents toward the non-Jewish society in which they live. As the betas in table 2 indicate, there are substantial differences in the political orientations and voluntary association activity of respondents with different denominational identifications, even when controls for demographic, socioeconomic, and Jewish identification variables are introduced.

The zero-order percentage differences between categories, which are not shown in table 2, are also quite large. Of the members of Reform synagogues, 50% have high levels of participation in non-Jewish voluntary associations, while 33% of Conservative members, 19% of the Orthodox, and 19% of the nonidentified have high levels of participation. The scores on the index of political liberalism also have an ordinal pattern. The nonidentified individuals are the most liberal (62% high), followed by the Reform members (49% high) and those nonmembers who identify with Reform (42% high). The Conservative members have only 29% with high scores, while the Conservative nonmembers have 23%; and 27% of the Orthodox received high liberalism scores.

Denominational Differences among Younger Respondents

Although the evidence for denominational differentiation and for the independent impact of denominational identification on ethnoreligious and secular behavior is clear, it might be argued that such patterns merely reflect differences in the degree of Americanization which have not yet disappeared from the American Jewish population. Proponents of this view could point justifiably to the strength of generation in the United States

as an explanatory factor in table 2. In order to see whether the patterns found for the total population were largely a function of cultural influences in the immediate past of American Jews, we repeated both types of MNA analyses for respondents aged 20-39, who are the least subject to such historic cultural influences, the most Americanized, and the most highly educated sector of the American Jewish population. Should the patterns which we found for the total population also be found among this younger subgroup, we may more safely argue for their endurance and importance within contemporary American Jewish life.⁶

Table 3 shows the prediction of denominational identification using the full set of NJPS variables for respondents from 20 to 39 years of age. The patterns shown there are very similar to those shown in table 1. The predictions for the Orthodox are poor, but the preferences of 86% of those having Conservative identifications, 65% of those identifying with Reform, and 72% of those having no denominational identification are correctly given. Thus, the predictions are more accurate for the Conservative and nondenominational groups than they were in the total population. The Reform predictions are similar, and only the Orthodox prediction is poorer. The poor prediction of Orthodox identification seems to reflect the fact that the younger Orthodox Jews are similar to the younger Conservative Jews in nearly all of their characteristics except religious behavior and piety.

Table 4 gives the beta coefficients on denominational preferences for the younger respondents. Two demographic variables, life cycle and education, have been dropped from the analysis because several of their subcategories are too small to permit stable analysis. The table shows that the denominational subgroups are somewhat more sharply differentiated with respect

TABLE 3
PREDICTION OF DENOMINATION USING MNA FOR RESPONDENTS
20-39 YEARS OF AGE

ACTUAL DENOMINATIONAL PREFERENCE	PREDICTED DENOMINATIONAL PREFERENCE (%)					N
	Orthodox	Conser- vative	Reform	No Preference	BASE	
Orthodox.....	9	85	3	3	100	131
Conservative.....	2	86	9	3	100	453
Reform.....	0	26	65	9	100	396
No preference.....	1	12	15	72	100	160

SOURCE.—See table 1.

⁶ Ideally, it would be better to limit such an analysis to American-born Jews. However, the additional requirement of being American born reduced the number of young Orthodox Jews to a mere 70, which is too few for stable MNA analysis.

TABLE 4

MNA BETA VALUES BY DENOMINATIONAL PREFERENCE FOR DEMOGRAPHIC, SES, AND JEWISH IDENTIFICATION INDICES AND FOR SECULAR CORRELATES, AMONG RESPONDENTS 20-39 YEARS OF AGE

	Orthodox	Conser- vative	Reform	No Preference
Demographic:				
Sex.....	.03	.10	.06	.06
U.S. generation.....	.15	.08	.12	.14
SES:				
Occupation.....	.09	.11	.17	.19
Income.....	.06	.07	.20	.11
Jewish identity:				
Jewish background.....	.10	.10	.11	.15
Jewish education.....	.02	0	.07	.08
Religious behavior.....	.18	.37	.31	.20
Pietism.....	.19	.07	.09	.07
Ideology.....	.08	.06	.04	.02
Primary group involvement.....	.07	.04	.15	.11
Jewish organizational involvement.....	.07	.07	.15	.17
Jewish child socialization.....	.06	.03	.09	.06
Concern for world Jewry.....	.03	.04	.04	.11
Secular correlates:				
General organizational involvement.....	.10	.07	.20	.08
Political liberalism.....	.12	.08	.04	.14

SOURCE.—See table 1.

to occupation and income among the younger respondents than they were for the total sample. Generational differences are still found, although they are naturally smaller than in the total sample. With controls for sex, generation, occupation, income, Jewish background, and Jewish education, the differences between the denominational subgroups in religious behavior are even stronger than those found in the total sample. Differences between the subgroups on several other indices of Jewish identification are smaller than in the total sample, but differences in Jewish primary-group involvement remain moderate. Differences in Jewish organizational involvement are even stronger than for the total population. Thus, the pattern with respect to ethnoreligious behavior seems to be one of continuing differences between denominational subgroups, particularly in the more organized, associational area of Jewish life.⁷

The results with respect to the two measures of secular orientations and behavior are quite similar to those for the total sample. The only real shifts are toward greater organizational activity among the Reform Jews and greater political liberalism among the Orthodox.

Although historical trends may not be safely deduced from cross-sec-

⁷ The squared multiple correlation coefficients for the MNA computations show slight shifts with a value of .25 for all Jewish respondents, .27 for synagogue members, .31 for nonmembers, and .34 for the young.

tional data, these findings on younger respondents do at least indicate that the basic patterns of religious and ethnic differentiations observed for the total American Jewish population also obtain among its younger, more Americanized elements. Most significant are the strong differences between the denominational groupings in religious behavior and the more moderate differences in Jewish organizational activity. These differences remain even with the introduction of multiple controls. Similarly, the impact of denominational identification on secular behavior observed within the total population is also found here.

CONCLUSIONS

After summarizing the implications of our data for the empirical issues raised at the beginning of the paper, we shall consider some of the implications for the study of American Protestant denominationalism.

As noted earlier, the social factors that helped generate and sustain the differentiation of the historic Protestant denominations in America and Europe have declined in force with the growing social and cultural homogenization of the American Protestant community. Nonetheless, there are still substantial socioeconomic and subcultural differences among the adherents to the various denominations. Furthermore, these differences have often been assumed to account for variations in the religious and secular behavior of affiliates. One recent development which has led observers to question the impact of national denominational affiliation on the individual is the growth of movements which cut across denominational lines. Moreover, recent studies have pointed to the differences in belief and behavior that exist within single denominations and sometimes even within single local churches. Studies of American Judaism have yielded analogous findings and have cast doubt on the significance of denominational affiliation.

Our data show that, as a result of socioeconomic and cultural homogenization, American Jewish denominations are only weakly differentiated in terms of the social and demographic characteristics of their members, with the important exceptions of their generation in the United States and the life-cycle characteristics of affiliates. However, there continue to be substantial differences between the denominational groups in the ethno-religious behavior and orientations of their affiliates. These differences may be weaker than they were historically, but they are still substantial. The three major denominational groupings are still ranked in the degree of traditionalism of their members, their ethnic loyalties, and religious observances. Those identifying with Orthodoxy rank highest on these dimensions, the Conservative Jews are intermediate in rank, and the Reform

lowest (Lazerwitz and Harrison 1979). Those individuals having no denominational preference are the most marginal to the Jewish community, its institutions, and its religious traditions. Predictably, when we consider involvement in non-Jewish organizations and political liberalism, the ranking is basically reversed.

Multivariate analyses reported here make it quite clear that the differences between the denominations in ethnoreligious orientations and behavior are not simply a function of the socioeconomic and demographic characteristics of those identifying and affiliating with the denominational groups. In fact, these patterns are largely independent of such individual characteristics. Additional evidence of the independent and enduring character of denominational differences is found when we look at the youngest, most Americanized sector of the Jewish population, those 20–39 years of age. Within this younger subgroup as well, these denominational patterns are equally prominent. One area in which no denominational difference was found is concern for world Jewry and Israel. These concerns have become a major focus of American Jewish life in both its secular and religious guises and a source of unity and group identity for the majority of American Jews (Liebman 1973).

In the area in which measures were available, we also found clear secular consequences of denominational affiliation and identification which are not simply functions of the socioeconomic and demographic characteristics of the individual affiliates. It seems clear, therefore, that at least within American Jewry, denominations *do* make a difference.

How do our findings compare with what is known about Protestant denominationalism, and what implications do they have for our understanding of the Protestant patterns? Our data suggest that it would be premature to ignore the social sources of denominationalism when examining American Protestants. If such factors as generation and life cycle continue to play a role within the more homogeneous American Jewish community, such key sources of differentiation as ethnicity, race, and region should continue to distinguish Protestant groups for some time to come. The greater socioeconomic and cultural heterogeneity of Protestant America should make these factors more significant in denominational differentiation than they are within American Jewry. On the other hand, if the evangelical and charismatic movements continue to grow within the mainline, middle-class churches, they will tend to blur the historic associations between religious orientations and social class background.

Although we do not have national data on Protestants that are strictly comparable with the NJPS data, the available studies all point to strong differences in belief and behavior among affiliates of denominations located within the different ideological camps. As Carroll and his colleagues (1977)

have suggested, it is still possible to array the denominations along a continuum from liberal to conservative in terms of their receptiveness to a publicly, socially oriented form of Protestantism as opposed to a more private, evangelical form (see Marty [1970], p. 178, on this distinction). In like manner, such findings as Klugel's (1979) point to the existence of distinct groups of nonaffiliates, liberals, and conservatives, with a middle group of moderates. This division and that suggested by Glock and Stark (1965) closely parallel the pattern that prevails within American Jewry. However, within American Jewry there are two analytically distinct but empirically correlated dimensions on which the denominational groupings may be ranked. One is emphasis on personal devotion and religious practice, while the other is emphasis on in-group loyalty and identification. The former dimension more closely parallels the one we might use in ranking Protestant bodies.

An additional aspect of our findings, important for the study of Protestant patterns, is the emergence of a distinctive group of individuals expressing no denominational preference. Klugel (1979) and Newport (1979) have found that individuals expressing no religious preference are drawn disproportionately from Jewish, Catholic, and high-status Protestant origins. Our data suggest that such individuals may retain some minimal ties to the socioreligious community from which they came, be it Protestant, Catholic, or Jewish. The ethnic and social ties of Catholics and Jews having no religious preference would help them sustain contacts with their original socioreligious milieus while abandoning formal religious affiliation or preference. Similarly, high-status Protestants may retain class-based ties to other liberal Protestants (e.g., Baltzell 1964) without maintaining religious affiliation. Diachronic data are needed for us to determine whether the heavy concentration of young people among those expressing no religious preference is truly indicative of the impact of secularization on youth, as Wuthnow (1976) suggests, or reflects their location in a phase of the life cycle which is less conducive to religious affiliation.

Can religious differences between Protestant denominations be expected to have secular impact to the extent found for American Jewry? The available literature on the subject suggests that we are unlikely to find such impact. There seem to be two reasons for the difference between our findings on American Jewry and those on the secular consequences of Protestant denominational affiliation. First, the minority position of American Jewry and its European origins have meant, historically, that Jewish Americans felt torn between in-group loyalty and adjustment to the host society. The Jewish denominations have been associated with different resolutions of this dilemma. The Orthodox have been the least assimilationist and the Reform the most, while the Conservatives have taken an

intermediate position. As a result, Jewish denominational identification is closely associated with those forms of secular behavior and orientations that bear on the issue of ethnic adjustment or resistance to the host culture. While Protestant denominational identification is not so closely associated with ethnic loyalties, it does bear some relation to the degree of loyalty to particular styles of behavior, especially in the case of the most conservative denominations, which have been strongholds of the evangelical and fundamentalist traditions. These traditions advocated a form of "private" Protestantism (Marty 1970) which stood in contrast to the more "public," socially concerned Protestantism of the liberal churches.

Thus, a second reason why studies of the consequences of Protestant denominationalism have produced few consistent findings may be that such studies have concentrated too heavily on political and economic behavior. Research is needed that examines the consequences of denominational affiliation in areas more closely tied to the church in American life—family patterns, leisure time use, values, and norms. These areas are more subject to individual discretion and more closely associated with the focal concerns of American religious institutions (Parsons 1960; Clebsch 1968).

In the light of these similarities and differences between Protestant and Jewish denominationalism, we may conclude in a more speculative vein with a comment on the position and functions of the denominational religious bodies that seem to be emerging in contemporary America. In offering these speculations, we assume that the levels of affiliation and attendance at established churches will not rise substantially during the next decade, that the demographic and cultural homogenization of the membership of the major denominations will continue, and that the search for new forms of spiritual and personal meaning beyond the boundaries of established churches will continue. Given these trends, conventional church affiliation will increasingly become simply one form of voluntary affiliation and self-expression among the many alternatives available to the individual.

Paradoxically, rather than spelling the end of the denominational churches, these trends may breathe new ideological vigor into them. As church affiliation ceases to fulfill a vaguely perceived social obligation, and as the choice of a church ceases to express clearly one's social standing and cultural background, ideological differences between the churches may become more, instead of less, salient to their affiliates. Subgroups of people who share distinctive socioreligious orientations and behavioral patterns may cluster increasingly around local churches that are officially or unofficially identified with such constellations of orientations and behavior.

As groups sharing a particular religious style become defined within denominations, coalitions of like-minded lay people and clergy may emerge

who pressure the national leadership to adopt policies and positions favoring theirs. In some cases splits are likely to occur between groups favoring and opposing a particular denominational image.⁸ Similarly, there would be a tendency for individual parishioners who oppose the drift of their churches to leave them, as occurred during the period of church activism in the 1960s (Wood 1972), while others will be attracted to the denomination's churches because of their distinctive position. Thus, denominations are likely to take on clearer religious and secular identities as individuals and groups "sort themselves out" on the religious and social issues of the day.⁹

This tendency will heighten their churches' ability to provide social support for parishioners' distinctive orientations and to buffer them against contacts with alternative ways of living and thinking (Berger 1969, pp. 147-49). The search for such subcommunities of meaning within the boundaries of established religion is likely to increase with people's exposure to more and more alternative interpretive schemes, value systems, and behavioral patterns and with the promulgation of the view that all of these alternatives are equally reasonable. Flooded by mass media with such a vast range of alternative prescriptions on how to live, and sensing that these alternatives are *not* all equally valid for them, some individuals will continue to seek in church affiliation a basis for ordering their personal priorities and for sustaining their beliefs that particular normative patterns are preferable. In the absence of natural communities based on ties of place, origin, or language to fulfill this function of providing meaning and social support, substantial portions of the population are likely to seek in the churches what Wilson (1968a) has termed "surrogate communities" (see also Greeley 1972). Given the ability of the American denominational churches to adjust themselves to their parishioners' social needs in the past, it seems probable that they will continue to function in the capacity of surrogate communities for some decades to come.

APPENDIX

Description of Variables

A. Jewish identification indices:

Childhood home Jewish background.—The Jewish aspects of a respondent's childhood home, covering items such as parental religious involvement, Jewish organizational activities, and home holiday celebrations.

⁸ Such a split occurred recently within the Missouri Synod of the Lutheran Church.

⁹ Johnson (1977) has made a somewhat similar suggestion, which we encountered after drafting our analysis.

Jewish education.—The type and amount received during childhood and adolescence.

Religious observance.—Respondent's present synagogue attendance, home religious observances of the Sabbath and the annual cycle of religious holidays, and observance of the dietary laws.

Pietism.—Observance of the more individualistic forms of religious expression, such as private prayers and fasting.

Jewish ideology.—Extent to which being Jewish and retaining Jewish values are felt to be desirable and intrinsically worthwhile.

Primary group involvement.—The extent to which a respondent's dating, courtship, friends, family life, and social life have been confined to Jews.

Jewish organizational involvement.—Extent of membership, activity, and leadership in Jewish voluntary associations.

Jewish socialization of one's children.—Degree of respondent's past, present, and anticipated efforts to socialize his or her children into Jewish life.

Concern for world Jewry.—Attitudes toward Israel and degree of concern over the fate of Jews in difficult circumstances in the rest of the world.

B. Secular correlates of Jewish identification:

Membership and participation in general community voluntary associations.—Here summarized by an index which is a slightly modified version of the Chapin scale.

Political liberalism.—An index of attitudes to such issues as school busing and aid to welfare recipients. More liberal responses received higher index scores.

C. Socioeconomic characteristics.—Respondent's education, the occupation of the head of the household, and total family income for the year prior to the survey.

D. Demographic characteristics.—Respondent's sex and age. Also, generation in the United States, ranked as: respondent foreign born; both parents foreign born; one parent foreign born; both parents born in the United States. The family life cycle categories ranged from single adults through young married couples, couples with preschoolers or children in elementary or high school, to middle-aged or elderly couples without children in their households, and included also one-parent households.

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Disagreement over the Evaluation of a Controlled Experiment¹

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Under the leadership of the U.S. Department of Labor, the states of Texas and Georgia engaged in what was perhaps the most careful large-scale randomized controlled experiment ever conducted in a natural setting. It was designed to test the widely supported theory that the recidivism rate of ex-convicts could be reduced or retarded by providing them for a time with a monthly monetary allowance. During the evaluation of the experiment, a radical difference of opinion over the correct interpretation of its results arose between the evaluators (Peter H. Rossi, Richard A. Berk, and Kenneth J. Lenihan) and Hans Zeisel, a member of the advisory board. This article, together with the comment and reply that follow it, explicates this difference and attempts to resolve it.

At issue in this paper is the proper relationship between the constraints imposed by the outcome of a controlled randomized experiment and the various modes of structural analysis aimed at clarifying the causal substructure of these findings.

Beginning in 1976, on an initiative from the U.S. Department of Labor, the states of Texas and Georgia conducted a large-scale randomized controlled experiment designed to find out whether providing a modest income to ex-convicts during the first months after their release from prison would prevent or at least retard their return to crime. The experiment was under the direction of Kenneth J. Lenihan, who had conducted a pilot study in Baltimore. Afterward, Peter H. Rossi and Richard A. Berk were asked to join Lenihan in the evaluation of the experiment. They published their conclusions in a book, *Money, Work, and Crime: Experimental Evidence* (Rossi, Berk, and Lenihan 1980) and simultaneously in an article in the *American Sociological Review* (Berk, Lenihan, and Rossi 1980). As a member of the advisory committee for that experiment, I found myself in such radical disagreement with its evaluators that I asked them to deviate from

¹ I am indebted to my colleague James H. Coleman for his critical reading of this paper. Requests for reprints should be sent to Hans Zeisel, University of Chicago Law School, 1111 East 60th Street, Chicago, Illinois 60637.

the time-honored formula for acknowledgments by recording the following statement in the introduction to their book: "At his request, we note here that in his opinion the approach taken in this manuscript is incorrect and that the conclusions we draw about the results of the TARP [Transitional Aid Research Project] experiments are severely flawed by methodological errors."

Our disagreement is simple and radical. Rossi and his colleagues believe the evidence to show that "limited financial aid to released prisoners . . . can decrease the arrests . . . in the year following release by 25% to 50%" (Rossi et al. 1980, p. 7). I believe the evidence shows nothing of the kind; on the contrary, the experiment proved that these payments failed to curb recidivism.²

Behind our disagreement on the issue of the experiment lies disagreement on the proper role of structural analysis in evaluating a randomized, controlled experiment. The present paper develops the reasons for my disagreement.

THE EXPERIMENT IN OUTLINE

Many experienced criminologists and prison officials have believed that one of the reasons many ex-convicts soon return to crime is that they lack income during the crucial first months after their release from prison. The rate of recidivism would slow down, so the belief went, if ex-offenders had some financial support during those first months. To test this belief, approximately 2,000 men and women about to be released from state prisons in Georgia and Texas were assigned randomly to four treatment groups and two control groups. The distinctions among the four treatment groups turned on the duration of the payments (13 or 26 weeks) and on various modes of reducing payments if the ex-convicts engaged in paid employment. The basic payment was \$70 a week in Georgia and \$63 a week in Texas.

The perfectly executed random assignment had the expected result: the demographic composition of each of the treatment groups and control groups was practically the same. And since the subsequent experimental treatments were also administered with care, this experiment was probably one of the best executed large-scale social experiments ever conducted in a natural setting.

² Loose language makes us talk of an experiment that failed. More precise language would distinguish between the failure of the treatment and the success of the experiment which divulged the failure. Howard Rosen, then the director of research and development in the Department of Labor's Employment and Training Administration, the prime mover of the experiment in question, understood the distinction. In a rare reversal of roles he, representing the sponsor of the study, was continuously concerned lest the payment plan be represented as more successful than it was.



The rearrest rate—the percentage of ex-convicts rearrested for a crime within one year after release—was the measure of success or failure of the experimental payments.

THE RESULTS OF THE EXPERIMENT CONCERNING ARRESTS

Figure 1 gives the composite result of the experiment by comparing the average rearrest rates of the four experimental groups with the rearrest rate in the control group.³

The global “no-difference” finding, of course, leaves open the possibility that any one or several of the four payment modalities reduced the rearrest rate while other treatments did not. Figure 2 shows that this expectation did not mature. The rearrest rates were practically identical for all four modes of payment.

The second possibility, that payments may have been effective to some extent, rested on the hope of finding a subgroup of ex-convicts for which the payment did in fact work. Conceivably, for instance, younger ex-convicts, or more intelligent ones, or those with family, or without family, might have responded to payments. It is essential that such a subgroup be identifiable by criteria available prior to the experimental treatment, even though the actual identification is made after the completion of the experiment. The importance of this point will become clear later on.

The reason that any such subgroup among the experimental subjects can

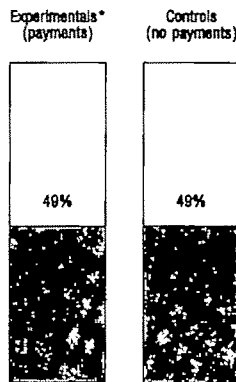


FIG. 1.—The global comparison: effect of payments on the rearrest rates among experimentals and controls. Asterisk indicates that the percentage is the Texas average for the four different modes of payment. (Source: Rossi et al. 1980, table 5.1, p. 93.)

³ The figures are for the Texas experiment. The figures for Georgia are statistically equivalent, even though the “no difference” was not actually zero, as it was in Texas. I have selected the Texas data because they make it easier to develop the methodological issue on which our disagreement turns.

be compared validly with its counterpart in the control group is that the total experiment can be seen as an aggregate of a number of limited experiments with any number of identifiable subgroups. In spite of their diligent search, Rossi et al. did not find such a subgroup.

In the present case, any finding of an effect in one of the subgroups would be under the constraint of the overall comparison (fig. 1) that showed no effect. If any of these subexperiments had shown a positive effect of the payments, by arithmetical necessity another subexperiment would have had to show a negative effect.

The clear result of the experiment is that none of the modes of payments affected the rate with which the ex-convicts returned to crime. Some returned earlier, some later, some not at all. Since no experimental subgroup of ex-convicts could be identified for which any of the payment modes showed a lower rearrest rate than appeared in the respective control group, the conclusion seems inescapable that whatever factors determined whether our ex-convicts returned or failed to return to crime, the payment was not one of these factors.

By what route, one may now ask, did Rossi et al. come to the opposite conclusion? The route begins with the experimental finding that the payments did have an effect, albeit not on the rearrest rate.

THE COUNTERBALANCING MODEL

The payments affected the amount of time these ex-convicts spent working during that first year after release. The experimentals, who received payments, spent less time working than the controls, who received none (Rossi et al. 1980, p. 17). In order to clarify a methodological problem, I have, with access to the TARP data provided me by Rossi et al., transformed the

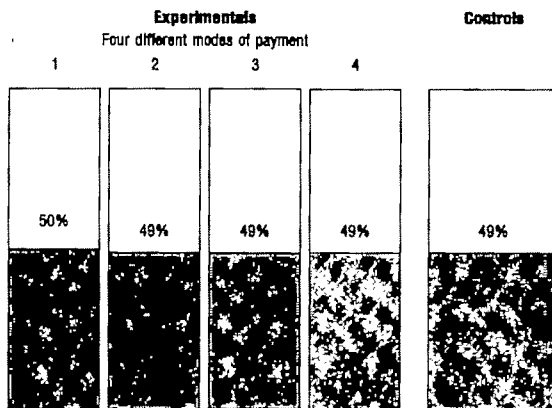


FIG 2.—Rearrest rates for four modes of experimental treatments and for controls

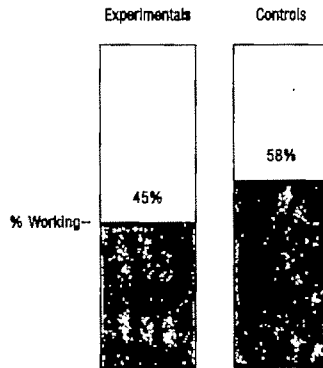


FIG. 3.—Effect of payments on time spent working; shaded area is percentage of ex-convicts working.

detailed data into a simplified fourfold figure (fig. 3). Here, time spent working is measured by a simple index: the percentage of ex-convicts in each group (experimentals and controls) who were working longer (or less long) than the median for all ex-convicts, a category that includes both experimentals and controls. Among the experimentals 45% worked longer than the common median time; among the controls 58% worked longer than the median. In addition, there was a countervailing effect not shown in figure 3: although the experimentals spent less time working, on the average they were in better-paying jobs than the controls.

The next step in the authors' structural analysis is no longer based on the result of the controlled experiment, but merely on a correlation: they found the amount of time spent working to be negatively correlated with the rate of rearrests.

The third step in the analysis combines the experimental finding that payments reduce working time with the correlational finding that working time and rearrests are negatively related and thereupon concludes that to some extent the experimental payments have caused the rearrest rate to rise above the level at which it would have been without the payments.

And now comes the surprising fourth analytical step. Since the third step showed an effect of the payments that increased the rearrest rate, and since the experiment showed no overall effect on the rearrest rate, the authors concluded that the difference between (1) the increased rearrest rate of the ex-convicts who worked less because of the payments and (2) the finding of no effect for all ex-convicts involved in the experiment must have been produced by a counterbalancing positive effect of the payments that depressed the rearrest rate.

This curious act of postulating, instead of proving, a balancing effect also explains, if that term is appropriate here, the surprising equivalence of

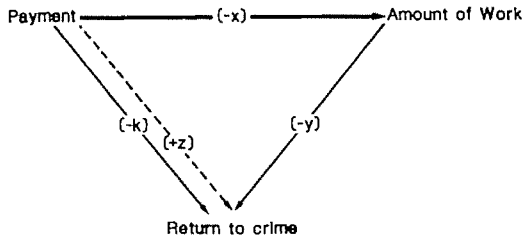


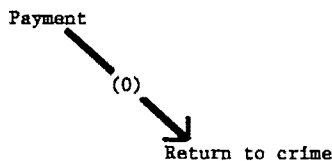
FIG. 4.—A simplified version of the path model developed by the experimenters (Rossi et al. 1980, p. 215; Berk et al. 1980, p. 774).

the two alleged effects, which net an overall effect of zero. It is on this postulated residual in the analytic chain that the authors rest their claim that financial aid to released prisoners can reduce the rearrest rate during the first year up to 50% and that the data, therefore, lend support to utilizing an income-maintenance strategy to reduce recidivism (Rossi et al. 1980, p. 7).

This theoretical framework is schematically represented by the path analysis in figure 4. The path derived from the controlled experiment is represented by a heavy line to distinguish it from the paths inferred through structural analysis. The dotted line represents the combined effect of the two lines with which it forms a triangle. The model was designed to represent these relationships:

1. payment reduces work $(-x)$;
2. reduced work increases return to crime $(-y)$;
3. through 1 and 2 payment increases return to crime $[(1 - x) \cdot (-y) = (+z)]$;
4. payment also reduces return to crime $(-k)$;
5. $(-k)$ and $(+z)$ happen to cancel one another and thereby produce the spurious appearance in the controlled experiment of a zero effect of payment on return to crime.

Note the absence of the path that represents the main result of the controlled experiment, namely, the path showing a zero effect of payment on return to crime. That path (shown in the unnumbered figure below) is not



there because the evaluators consider it a spurious result brought about by the coincidence that $(-k)$ and $(+z)$ cancel one another. This strange equality of (k) and (z) , as we have pointed out above, is not derived from

independent numerical estimates of the two values but simply by postulating that the difference between zero and $-k$ is the correct measure of the alleged power of the payments to reduce the return to crime.

THE AMBIGUITY OF CORRELATIONS

One pivot on which our disagreement turns is the way we do or do not distinguish between evidence derived from the randomized experiment on the one hand and evidence derived from correlations and their more complex derivative—the structural equation model—on the other hand. The two research instruments differ in important respects.

The controlled, randomized experiment allows us to attribute differences and nondifferences in outcome (within the limitations of the sampling error) unequivocally to the experimental stimulus, because, except for that stimulus in the experimental groups and its absence in the control group, the groups can be presumed to be indistinguishable.

Correlational evidence is different in that it requires a theoretically endless chain of safeguards and assumptions to arrive at reasonably plausible causal inferences. It is the essence of the controlled experiment that no such subsequent safeguards are needed because they have been built into its design. The difference between the two types of evidence with respect to causal inferences is thus a difference in kind, not one of degree.

In the present instance, the essential ambiguity of inferences from correlations can be demonstrated best by showing how the same data interpreted by a different theory lead to a radically different interpretation. Although it is sufficient for my argument to show that an alternative model can be developed from these data, I will suggest that this alternative model may even be superior to the model proposed by Rossi et al. (1980, p. 106).

The data on which the following exposition is based come from the experiment at issue, albeit slightly transformed. I have compressed them into dichotomies which allow a simpler presentation of the analytical logic. Since only the logic and not the numerical refinement is at issue, this transformation simplifies the argument without altering it.

We begin with the two fourfold tables of figure 5, which show the relation of working time to rearrest separately for experimentals and controls. The rearrest figures provide natural dichotomies: those rearrested versus those not rearrested. The work figures represent the artificial dichotomy described above. The numbers refer to the Texas experiment; the Georgia numbers would have served just as well, except that for our purpose they had more "noise."

We now transform the numbers from figure 5 into the bars of figure 6. The first column of figure 5 becomes the second bar of figure 6; the second

column of figure 5 becomes the last bar of figure 6; the third column of figure 5 becomes the first bar of figure 6; the fourth column of figure 5 becomes the third bar of figure 6.

Figure 6 can be read to show that payments have a positive rearrest-reducing effect both on ex-convicts with a strong desire to work and on those with a weak desire to work. The interpretation of "working more" as indicator for a "strong desire to work" (and vice versa) was made by me to suggest more persuasively the direction of the causal chain. Desire to work is the ex-convict's state of mind, a condition that will determine the effect of the payments. With ex-convicts who have a strong desire to work, payments reduce the arrest rate by one-third or 11 percentage points. With ex-convicts who have a weak desire to work, the decline is smaller: a reduction by one-fifteenth or 5 percentage points.

Figure 6, of course, proves nothing of the kind because it is only in form, not in substance, the replication of an experiment. The headings

	Experimentals			Controls		
	Worked	Did Not Work	Total	Worked	Did Not Work	Total
	Not Rearrested	52	239	146	19	165
	58	252	310	77	144	221
Total	245	304	549	223	163	386

FIG. 5.—Relationship between time spent working and rearrest rates among those who received payments (experimentals) and those who did not (controls).

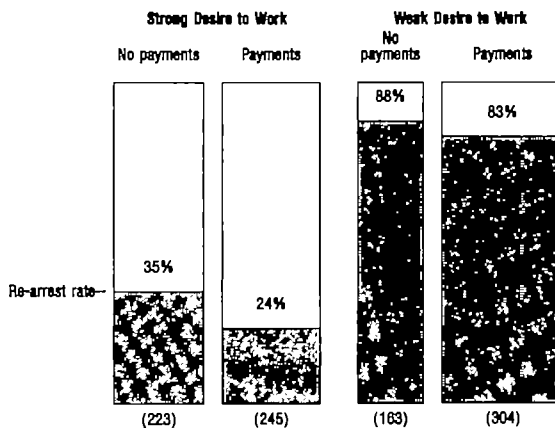


FIG. 6.—"Effect" of payments on rearrest rates among ex-convicts with strong and weak desire to work.

over each pair of bars give the impression that the subjects in the two bars are comparable; they are not, because "working—not working" was an outcome variable of the experiment, not a preexperimental randomized characteristic.

The frailty of the causal inferences we have drawn hypothetically from figure 5 becomes obvious if we transform the numbers of figure 5 in a different fashion into figure 7. Here we assume "being rearrested" as indicating a mental condition that precedes the experiment; having been rearrested is taken as indicating strong criminal tendencies, not having been rearrested as indicating weak criminal tendencies. Figure 7 is designed to answer the question of how the payments affect the work of ex-convicts who have strong or weak criminal tendencies. Payments, it appears, are a disincentive to work for both types of ex-convicts, those with strong and those with weak criminal tendencies. The disincentive is greater with ex-convicts having strong criminal tendencies.⁴ Figures 6 and 7, the two versions in which the numbers of figure 5 are presented, represent two different theories about the causation of crime. Figure 6 represents the economist's view of the criminal as someone who rationally considers opportunity costs as if crime were just another economic transaction. Figure 7 represents the view that by the time an adult criminal has received and completed a prison sentence his criminal character is set, so that money payments are not likely to change his course.

The correlation data themselves do not enable us to decide which of the

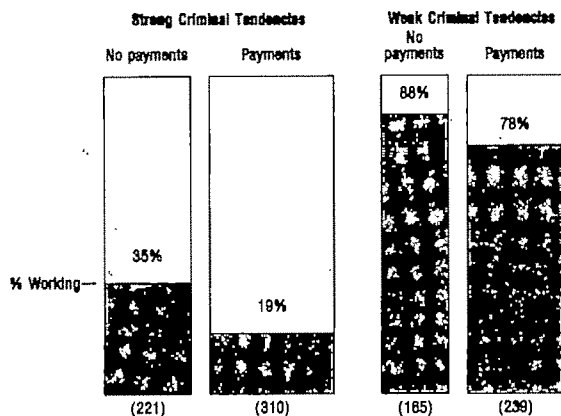


FIG. 7.—"Effect" of payments on time spent working by ex-convicts with strong and weak criminal tendencies.

⁴ For this discussion of the ambiguity of correlational data, it is irrelevant that the work-disincentive effect of the payments happened to be proved by the controlled experiment.

two theories, that is, which causal chain, is the correct one. Given this predicament, I am nevertheless inclined to believe that Rossi et al.'s preference for the paradigm of the economist (fig. 6) may be misguided. Three considerations make me prefer the paradigm of figure 7, in which "criminal tendency" is the intervening causal variable.

First, from all the studies I have seen and have made myself, I have tentatively concluded that becoming a criminal is an event that occurs early in life and is hard to undo later. Accordingly, the path analysis presented in figure 8 seems more appropriate. It represents these propositions: (1) payment reduces work ($-x$); (2) payment does not affect return to crime (0); and (3) return to crime reduces work ($-y$). Second, for the inferred relationship, 3 there is support in the data themselves. They provide us with two correlations between return to crime and work, one for the convicts who received payments and one for those who did not.

As shown in table 1, the effect of return to crime on work is of the same magnitude regardless of whether or not the subjects received payments.⁵ Third, this model (fig. 8) has the advantage of incorporating both findings (represented by the heavy lines) from the controlled, randomized experiment.

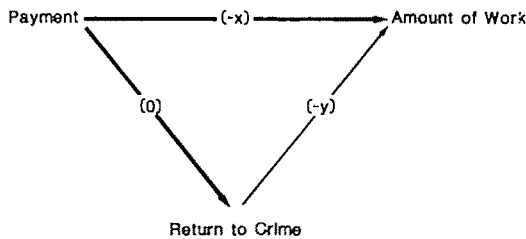


FIG. 8.—Relationship between payment, amount of work, and return to crime

TABLE 1
RELATIONSHIP BETWEEN WORK AND RETURN TO CRIME

	RECEIVED PAYMENTS		DID NOT RECEIVE PAYMENTS	
	Returned to Crime	Did Not Return	Returned to Crime	Did Not Return
Percentage working in each group	19	78	35	88
Difference	+59		+53	

⁵ The log-linear transformation yields even closer values for the two comparisons: 2.61 for the convicts who received payments, 2.74 for those who did not.

My disagreement with Rossi et al. on this aspect of the problem, however, is collateral. It is different in kind from my insistence that they have misread the main evidence of the experiment by disregarding the crucial distinction between a controlled experiment and a correlational structural model. To interpret correlational data a theory is needed, and unless the theory is correct the interpretation will not be correct. It is the beauty of the controlled experiment that all the theorizing goes into its design. The result of the experiment, though more limited in its scope than correlational speculation, speaks for itself and needs no further theoretical support.

OBLITERATING THE DISTINCTION

The authors do not respect this distinction. Nowhere is the reader advised that there is a difference in kind between the evidence gained directly from the controlled experiment and the evidence gained from the structural equations. The honorific mantle of experimental evidence is allowed also to cloak the evidence which is not experimental.

To begin with, the subtitle of the book is *Experimental Evidence* even though three-fourths of its analytic content is devoted to nonexperimental evidence. Then there is the statement in the introduction that "the TARP findings are especially trustworthy because of the research design employed. Randomized controlled experiments are the most powerful techniques available for the assessment of the effects of social programs" (Rossi et al. 1980, p. 7).

The disregard of the distinction between experimental and nonexperimental evidence reaches its high point in the article (Berk et al. 1980) published simultaneously with the book. It is essentially a condensation of the book with one interesting modification. Of its 20 pages only one-third of one page, halfway through the article, is devoted to the experimental findings, and it begins in this way: "In the interest of space, we have not included tables showing the reduced-form results for Texas and Georgia. With two important exceptions, the substantive findings from the reduced forms are uninteresting, especially in the context of the structural-form results . . ." (p. 777).

At this point, it will be useful to state what I consider the justifiable conclusions from the experiment:

1. None of the four experimental forms of paying unemployment insurance to ex-convicts served to reduce the rate of their recidivism.
2. It was not possible to identify any subgroup of ex-convicts for whom such payments proved to be effective.
3. These payments had the effect of reducing the number of weeks

worked by the ex-convicts during the year following their release, and enabled them to spend these fewer work weeks on better-paying jobs.

4. The nonexperimental evidence shows a general negative correlation between the number of weeks worked and the rate of recidivism. It is, therefore, possible that a payment that would not be a disincentive to work might reduce the rearrest rate. Whether such a mode of payment exists can be learned only from another controlled experiment.

There is one last, simple way of showing how tenuous, at best, the authors' claim is that the payments reduce recidivism. On the basis of what they have learned from this experiment, even including knowledge they have gained from structural analysis, there is no way they can fashion a valid prescription taking the form: "Such-and-such payments to offenders recognizable by such-and-such characteristics will result in a net reduction of rearrests."⁶

This is the pragmatic proof that a structural model which is incompatible with the results of a controlled, randomized experiment has little claim to represent the truth.

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⁶ If a future experiment should discover and identify a subgroup of ex-convicts for whom payments would reduce the rate of rearrests, an interesting problem would arise: Would our Constitution allow making payments to that group and withholding them from other ex-offenders?

Saying It Wrong with Figures: A Comment on Zeisel¹

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Because of the respect and admiration we have for Hans Zeisel's long and distinguished career, it is especially troubling to have to respond to a paper which can only subtract from the sum of his accomplishments. But there are so many errors and misunderstandings in his article that we are forced to point them out lest readers of his critique be led astray.

To begin with, there are several misstatements in the article, some more important than others. These follow. (1) Zeisel did *not* ask us to add a statement in the acknowledgments of *Money, Work and Crime* recording his disagreement. Instead, he asked that his name be withdrawn from the list of members of our advisory committee in the volume's acknowledgments. The statement was written by us out of a desire to avoid rewriting history, because he had served long and faithfully on that committee and because we wanted to accord appropriate respect to his disagreement. (2) Instead of four payment groups, as Zeisel appears to believe, there were only three in the experiment (a point which is repeated many times throughout the monograph). (3) At many points throughout the monograph we emphasize that the payments *as administered* had no effects on recidivism. The quotation presented by Zeisel as our statement of the findings is followed in the original text by qualifying statements that clearly point out the zero-order null effects of the payments as administered. Contrary to Zeisel's claim, neither our monograph (Rossi, Berk, and Lenihan 1980) nor our article in the *American Sociological Review* (Berk, Lenihan, and Rossi 1980) contained attempts to obscure the finding that the treatments failed to achieve their desired effects. (4) The experiment did not test payments, as Zeisel suggests, but only payments as administered.

¹ Requests for reprints should be sent to Peter H. Rossi, Social and Demographic Institute, University of Massachusetts, Amherst, Massachusetts 01003.

(5) Zeisel conveniently ignores the evidence of the LIFE (Living Insurance for Ex-prisoners) experiment, a small-scale precursor of the Transitional Aid Research Project (TARP) in which payments administered under different rules did have direct effects lowering recidivism and had little or no work disincentive effects. The LIFE experiment is discussed in great detail in the monograph and forms part of the evidence we cite for our conclusions concerning the counterbalancing model. (6) The diagram shown in Zeisel's figure 4, which he claims to be a "simplified" version of the TARP counterbalancing model, is simplified beyond recognition. One of the paths in the model that completes the representation of reciprocal effects between payments and work, critical to our interpretation and a feature of the experimental design, has been omitted, as are the indirect paths that feed back from arrests to work and payments. (7) Zeisel's summary of the current state of criminological knowledge is simply uninformed. Among the many facts that are at variance with the claim that "becoming a criminal is an event that occurs early in life and is hard to undo later . . ." is that the majority of offenders released from state prisons do *not* return to prison. Furthermore, participation in crime declines dramatically after the age of 25, hardly evidence for the fixed nature of criminal character. The finding that crime rates are also sensitive to short-term economic cycles (Cohen and Felson 1979; Cohen, Felson, and Land 1980) shows that crime can also be influenced by external events.

Much more seriously misleading than the foregoing errors and misunderstandings are Zeisel's attempts at model building. His models have the virtue of being simple and easily understood but also the overwhelming defect of being simply wrong, based on outmoded ideas and serious misunderstanding of the nature of model building. Although the gap between his procedures and modern analytic techniques cannot be bridged effectively here, we can provide at least some comments on some of the major defects.

1. As we stated clearly a number of times, the counterbalancing model and related specification were postulated before the structural equations were estimated and were constructed both on the basis of the result of the prior LIFE experiment in which a payment plan did have a recidivism reducing effect and by drawing on social science theory concerning the possible competition between legitimate and illegitimate income producing activities. There is a vast difference between posing a theory before examining the data and posing one after examining them. On that basis alone, Zeisel's post hoc models cannot stand on equal footing with our a priori model. Furthermore, our modeling was tested through replication, being first constructed on the data derived from the Texas experiment and then tested on the data from the Georgia experiment.

2. Zeisel is correct when he observes that with null main effects in a

randomized experiment the only way the treatment can be positively effective for one subgroup is for it to be negatively effective for another. This reasoning applies not only to subgroups but also to processes, an essential component in our model. The TARP model postulates counterbalancing processes, impossible to model with the polytomous tables that Zeisel uses in table 1 and in figures 5 through 8.

3. The effects we claim to have shown with the TARP modeling are certainly not proved definitively. Indeed, we propose in our monograph an additional series of experiments to test whether or not the work disincentive effects of payments can be avoided in payment plans that do not tie benefits to unemployment. The estimated effects are consistent and roughly of the same magnitude in the two separate experiments, one each in the states of Georgia and Texas, that make up the TARP project. They are also consistent with a significant body of microeconomic theory and with knowledge derived from occupational sociology. They are also in accord with findings of the previous LIFE experiment.² Indeed, the reason that so much of our monograph and our article is devoted to the counterbalancing model as opposed to the main null effects is that we felt a responsibility to our readers to show in detail the reasoning and calculations that went into the specification of the model and the estimation of its parameters.

The models suggested by Zeisel as competitors to the TARP counterbalancing models have very little in the way of comparable evidence to support them. Heuristic examples would be instructive, but not if they might mislead a reader into thinking that any model, however absurd empirically or theoretically, is as valid as any other model. It is absurd to propose, as a proxy for liking to work, whether or not a person worked more than the median weeks of work and to propose, as a proxy for criminal tendencies, whether or not a person is arrested. Such proposals are even more irresponsible inasmuch as the original TARP data (to which we offered Zeisel access) contain many variables (e.g., summaries of prior criminal records, whether or not arrangements were made for postrelease jobs when in prison) that are patently better proxies and that were measured before randomization. In short, an exercise in repercentaging tables is nothing more than a demonstration of primal arithmetic and certainly far from a responsible essay in model building.

² Recent work in California provides further support for the view that when work disincentives are reduced, unemployment benefits (and similar kinds of transfer payments) can reduce recidivism. California currently has on the books legislation (Senate Bill 224) that allows prisoners to earn eligibility for unemployment compensation from prison jobs and vocational training. Using a regression-discontinuity design that achieves the same kind of orthogonality that characterizes randomized experiments (Barnow, Cain, and Goldberger 1980), Berk and Rauma (in press) find that recidivism rates are cut by about 10%.

4. Zeisel's claim that we obliterated the distinction between a controlled experiment and a correlational structural model is simply uninformed. The experimental manipulation was incorporated into our model as an unambiguously exogenous variable. Indeed, the crucial path which Zeisel omitted from his simplified version of the TARP model (fig. 4) is a feature of the experimental manipulation and not postulated by us. The structural equations of the TARP model do not rest on mere correlational data, as Zeisel asserts; they rest instead on a quasi-experimental design using nonequivalent control groups.

In summary, we would be the first to agree that given the current state of social science theory, structural equation results are unlikely to be as convincing as results from randomized experiments. However, in the TARP analysis, the reduced-form equations representing the randomized experiment results and the structural equations representing the counterbalancing model speak to different questions. The reduced-form equations show that payments as implemented in the TARP experiment did not produce net reductions in recidivism. *There is no dispute about this conclusion.* The structural equations explore *why* null effects were found, and the data are consistent with a view that work disincentives associated with the payments as administered in TARP masked what otherwise would have been encouraging reductions in the rates of rearrest (as was shown in the previous LIFE experiment). Thus we stand firmly behind the quotation with which Zeisel disagrees. Our analysis indicates that payments that do not incorporate heavy work disincentives can reduce recidivism. Only additional experiments that attempt to minimize work-disincentive effects can provide highly convincing evidence about the validity of the last point; however, we believe that the preponderance of the evidence from LIFE and TARP bolsters that viewpoint.

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Hans Zeisel Concludes the Debate¹

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The points Rossi, Berk, and Lenihan have selected for their comment, and the points in my article they have not replied to, reflect, if I am not mistaken, a considerable shift on their part toward agreement between us. I have tried to formulate that below. First, however, I want to clarify a few misunderstandings which otherwise might obstruct the view of what we may agree on. First, three minor points:

1. The reproach that I talk of payments generally instead of payments as administered is unwarranted. Here, there is no difference between us. My reference to payments always denotes the payments administered in the TARP experiment; there were no others. It is one of the limitations of a controlled experiment that it tests only the administered treatments and by definition deals with others only through extrapolation.

2. Rossi et al. write, "The structural equations of the TARP model do not rest on mere correlational data, as Zeisel asserts; they rest instead on a quasi-experimental design using nonequivalent control groups." In the presence of a powerful and carefully controlled randomized experiment, I took the liberty of calling all not-controlled evidence "correlational." This dichotomy emphasizes my main point: that in the presence of a controlled experiment, nonexperimental evidence cannot subvert the experimental finding.

3. Rossi et al. chide me for writing about four payment groups when there were only three. My error, if it is one, derives from page 49 of their book (Rossi et al. 1980), where they state: "The experimental design . . . was as follows: Group 1: . . . Group 2: . . . Group 3: . . . Group 4: . . . Group 5: . . . [a control group]"

Then there are three major points that go to the substance of the debate:

1. Rossi et al. reproach me for neglecting the evidence from the pilot LIFE experiment, which, they point out, "is discussed in great detail in the monograph and forms part of the evidence we cite for our conclusions." That experiment Rossi et al. themselves describe as a "small-scale precursor" of TARP. It was the statistical hint derived from that experi-

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ment which spawned the subsequent major effort. Once that effort was made, the modest hint of an effect in the pilot study loses whatever evidential power it may have had. For what it is worth, I warned at the time against taking too seriously the modest veering in the hoped-for direction in the LIFE experiment.

2. Concerning my discussion and simplification of the various models, my main point was that even the correlational data derived by Rossi et al. permit a different interpretation from the one Rossi et al. gave. I happen to believe that mine is closer to the truth, but such preference is a minor point. The main point is that these data are ambiguous. My reducing the correlation matrix, in this context, to a dichotomous form (for which the data, incidentally, were provided by Rossi et al. at my request) does not alter the argument, but merely simplifies its presentation.

3. The sequence in which model theory and model data were developed is of interest but irrelevant to the question whether the model describes reality.

Finally, Rossi et al. make a number of criticisms in which the *ad hominem* tone of the argument outweighs its substance. Of these I will merely answer one, and that only because Rossi et al. themselves have answered it.

Their very first allegation of "several misstatements" on my part is that I had *not* (their emphasis) asked Rossi to add a statement in the acknowledgments recording my disagreement but had merely asked that my name be withdrawn from the list of members of the advisory committee. Yet on page xxii, in the acknowledgments of their book (Rossi, Berk, and Lenihan 1980) one can read: "*At his request*, we note here that in his opinion the approach taken in this manuscript is incorrect and that the conclusions we draw about the results of the TARP experiments are severely flawed by methodological errors" (emphasis added).

As to Rossi et al.'s claim that there were no "attempts to obscure the finding that the treatments failed," I refer the reader to the quotations in my paper and to the book from which they were taken (Rossi et al. 1980).

I now come to the central issue in this debate, namely, whether it has ended as any debate should in substantial agreement about what has been resolved and what remains to be clarified. The need for such resolution was my main reason for starting this debate in the first place.

At issue is one of the best controlled social science experiments—if not the best—ever conducted in a natural setting. Yet scholars disagree sharply on its evaluation. This seems to me an intolerable situation because it invites general contempt for social science research, especially from the courts, who ever more frequently are asked to accept social

science research as evidence. If the professors cannot collate their interpretations, the prestige of social science is bound to suffer. For that reason, I would consider it a fitting ending to this debate if the following points might constitute, as I hope, a fair summary of the now clarified position of Rossi et al.:

1. The payments as administered in the TARP experiment failed to reduce the average recidivism rate.

2. The reason for this, we believe, is that the payments had a positive (reducing) effect on some ex-convicts and a negative effect on others.

3. If one could devise a mode of payment that would eliminate the negative effect, the average recidivism rate would drop.

4. At this time, we are unable to specify payments that could be relied upon to achieve that goal, either in terms of a different mode of delivery or by designation of a recognizable group of ex-convicts who would be affected positively.

5. From our knowledge of what causes crime, and from certain structural analyses performed in this study, we are convinced that we will find a payment mode that will reduce the average rate of recidivism.

6. Such a new plan can then be submitted to another test through controlled experimentation. At this time, however, we have no such plan.

If this is a fair summary of their position, then the debate has yielded the hoped for clarification and agreement.

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The Association between Husbands' and Wives' Occupations in Two-Earner Families¹

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The structure of a society's status hierarchy is usually investigated through the analysis of occupational mobility, that is, the association of fathers' and sons' occupations. This paper introduces another indicator that is conceptually similar but empirically distinct: the association of husbands' and wives' occupational statuses. Five important findings emerge: (1) the association is symmetrical—it does not matter whether husband or wife is taken as referent when assessing the association; (2) most of the association is due to status consistency between husbands and wives; (3) the strongest status effect separates lower nonmanual from blue-collar occupations; (4) the compound effect of consistency and status effects is to produce strong status-group boundaries separating farm from nonfarm and manual from nonmanual occupations; (5) a comparison with a father-son mobility table reveals a striking amount of similarity, reinforcing the view that the husband-wife association and father-son association tap the same underlying structure.

A society's occupational distribution is a crucial component of its status hierarchy. Occupation per se is not status, but rank in the occupational hierarchy is an important determinant of status. Status hierarchies are not all alike. Even though all hierarchies engender at least some level of inequality, societies in different periods vary in the relative prevalence of desirable positions and the circulation of society's members among positions. Traditionally the sociologist's primary tool for the analysis of these attributes of status hierarchies is the occupational mobility table—the cross-classification of the occupations of current job holders by the occupations of their fathers (or occasionally, their mothers). These mobility tables indicate the links between families of two generations. The emer-

¹ This research was supported by the University of Arizona. I wish to thank Albert Bergesen, Clifford C. Clogg, Otis Dudley Duncan, Robert M. Hauser, Robert A. Johnson, Patricia MacCorquodale, and Arthur L. Stinchcombe for their helpful comments on an earlier draft of this paper. Of course, I alone am responsible for my use of their comments. This paper was presented at the annual meetings of the Population Association of America, Washington, D.C., March 26–28, 1981. Requests for reprints should be sent to Michael Hout, Department of Sociology, University of Arizona, Tucson, Arizona 85721.

gence of the two-earner family as a common arrangement in modern society offers the possibility of analyzing the status hierarchy as it is manifested in the links between the occupational positions of members of the same family in one generation.

In 1977, both spouses were employed in 60% of all husband and wife families in which the husband was less than 55 years old (U.S. Bureau of the Census 1980*b*, Table 748). The occupational statuses of both spouses make independent contributions to family status. Experimental evidence (Rossi et al. 1974; Sampson and Rossi 1975) indicates that although the effect of the wife's occupational status on family status is less than the effect of her husband's (by about 50%), it is greater than the effects of both spouses' educations and of race. Even though husbands' and wives' contributions to family status are independent of each other, it is unlikely that the occupations themselves are independent. The status boundaries that constrain the joint distribution of fathers' and sons' occupations are likely to be important for the occupations of husbands and wives, too. For that reason knowing the association between husbands' and wives' occupational statuses is important for our understanding of the workings of status in modern society.

The basic questions underlying the study of intergenerational mobility concern the rigidity of the status hierarchy and the importance of status-group boundaries in the overall process of occupational achievement. The same questions can be addressed through the analysis of the association between husbands' and wives' occupations. The questions of rigidity and status-group boundaries refer to the strength of the association and are independent of the marginal distribution of occupations in society. The marginal distribution is a function of the division of labor and can change without altering hierarchical rigidity and status-group boundaries. Therefore issues of distribution must be kept distinct from issues of association. The unlikely event of there being no association between spouses' occupational statuses would indicate an openness of the status hierarchy not evidenced by studies of the association between fathers' and sons' statuses. On the other hand, a positive association would indicate that status boundaries constrain the occupational choice of individual family members.² Given the other evidence of the generality of the status hierarchy in the

² Although evidence is lacking, several microprocesses are likely to contribute to an association. Individuals may rule out occupational possibilities too different in status from their spouses' occupations. Information about employment possibilities brought home by an employed spouse may steer job seekers toward related occupations (although it is not certain that the information will be only about occupations of similar status). Large status differences between spouses may lead to marital disruption, selecting out of the population of husband and wife families instances of the most discrepant statuses and leaving behind a population selected for high association between husbands' and wives' statuses.

United States (Duncan and Duncan 1955), null association must be regarded as a remote possibility. The real empirical questions concern the strength and form of the association between husbands' and wives' occupations.

Family formation processes supplement the influence of general stratification processes in determining the extent of association between husbands' and wives' occupations. Race, education, and status of family of origin influence not only occupational placement (Blau and Duncan 1967; Featherman and Hauser 1978) but also the choice of a marriage partner (Ramsøy 1966; Blau and Duncan 1967; Peach 1974). A decomposition of the association between husbands' and wives' occupational statuses into the portion arising from assortative marriage and the portion due to status constraints on occupational placement is beyond the scope of this analysis. Suitable modifications on Johnson's (1981) models of religious assortative marriage would serve as a good starting point for such a decomposition. Nonetheless, whether the association between husbands' and wives' occupations arises from family formation or from the stratification process per se, it is a reflection of the underlying status group structure of society.

In this paper, I first specify a model of the association between husbands' and wives' occupations and then compare that association with the association between fathers' and sons' occupations.

DATA AND METHODS

The data for this analysis are from a table which cross-classifies husband's occupation by wife's occupation for families in which both were employed in March 1978 (U.S. Bureau of the Census 1980a). Estimates of sample frequencies were calculated from population estimates using the published standard errors for Current Population Survey (CPS) samples and the formula: $N = p(1 - p)/SE^2$ where p is the proportion in the cell to be estimated and SE is the standard error (see B. Duncan 1979). This formula gives the cell count that would produce the SE had simple random sampling been used. In that sense the frequencies in table 1 are "effective counts" for the purposes of testing the fit of proposed models. The sampling design of the CPS is complex, and some latitude must be given in the interpretation of test statistics, but the procedure is preferable to the use of population estimates.

A five-category classification is used for occupation. The five categories (in order of descending status) are: (5) professional and managerial workers; (4) sales and clerical workers; (3) craftsmen, foremen, operatives, and nonfarm laborers; (2) service workers; and (1) farmers, farm managers, and farm laborers.

DATA ANALYSIS

The first column of table 1 presents the effective counts for the 5×5 cross classification of husband's occupation by wife's occupation for couples in which both spouses were over 15 years old in March 1978. The analysis employs the methods for ordered tables discussed by Goodman (1979*a*, *b*; see also O. D. Duncan 1979). Table 2 summarizes the fit of the models to the data. Fit is gauged by the likelihood ratio $\chi^2 : L^2 = 2\sum_{ij} f_{ij} \ln(f_{ij}/F_{ij})$ where f_{ij} is the observed frequency and F_{ij} is the frequency expected under the model. The terms are those used by Goodman (1979*b*); the reader is referred to that source for details on specifying and fitting the models. Test statistics and parameter estimates for models 7 and 9 were obtained using the algorithm outlined in Goodman (1979*b*).³ All

TABLE 1
OBSERVED AND EXPECTED FREQUENCIES FOR CROSS-CLASSIFICATIONS OF
HUSBANDS' AND WIVES' OCCUPATIONS (March 1978) AND FATHERS'
AND SONS' OCCUPATIONS (March 1973)

OCCUPATION OF HUSBAND OR FATHER	OCCUPATION OF WIFE OR SON	CROSS-CLASSIFICATION			
		Husband-Wife		Father-Son	
		Observed	Expected Model 8	Observed	Expected Model 8
5.....	5	1,708	1,708	1,392	1,392
	4	1,872	1,877.55	791	843.25
	3	236	230.34	1,105	1,063.34
	2	416	422.03	113	109.38
	1	26	20.08	60	53.02
4.....	5	405	398.61	519	469.25
	4	836	836	482	482
	3	142	145.31	633	677.56
	2	192	192.86	57	59.25
	1	3	5.22	28	30.95
3.....	5	741	751.68	1,227	1,253.63
	4	2,242	2,233.67	1,465	1,435.48
	3	1,297	1,297	5,471	5,471
	2	1,125	1,121.68	407	401.75
	1	34	34.97	301	309.14
2.....	5	149	135.44	180	206.16
	4	279	291.53	243	200.67
	3	112	110.30	622	642.28
	2	245	245	126	126
	1	3	5.73	43	38.89
1.....	5	62	71.27	385	381.95
	4	97	87.25	381	400.60
	3	34	38.05	1,912	1,888.83
	2	67	63.43	142	148.62
	1	114	114	1,832	1,832

NOTE.—Occupational categories are (5) professional and managerial workers; (4) sales and clerical workers; (3) craftsmen, operatives, and nonfarm laborers; (2) service workers; and (1) farmers and farm workers.

³ Programming was done by Clifford C. Clogg, who generously provided me with the model II results reported herein.

other models were fit using the computer program FREQ from Haberman (1979, pp. 571-85).

The preferred model for the husband-wife association is the homogeneous row and column effects (I) model (model 8). According to this model, the association between husbands' and wives' occupations depends on which combinations of occupations are being examined, but the association is homogeneous, in other words, the effect of status change for the wife on her husband's status is equal to the effect of change in his status on hers. This homogeneity is important. It means that while the occupational categories in table 1 are not evenly spaced along an interval scale,

TABLE 2
ANALYSIS OF FIT OF MODELS OF ASSOCIATION
APPLIED TO HUSBAND-WIFE DATA
A. MODELS OF ASSOCIATION

Model	df	L_m^2	L_m^2/L_1^2
1. Independence.....	16	2,263.38*	1.000
2. Quasi-independence*.....	11	364.14*	.161
3. Uniform association.....	10	151.78*	.067
4. Row effects*.....	7	78.70*	.035
5. Column effects*.....	7	111.45*	.049
6. Row and column effects (I)*.....	4	5.30	.002
7. Row and column effects (II)*.....	4	14.73*	.007
8. Homogeneous row and column effects (I)*.....	7	9.78	.004
9. Homogeneous row and column effects (II)*.....	7	19.38*	.009
10. Symmetric association.....	6	7.85	.003

B. COMPARISON OF MODELS

TEST SIGNIFICANCE OF:	MODELS		df	$L_m^2 - L_{m'}^2$	$(L_m^2 - L_{m'}^2)/L_1^2$
	m	m'			
A. Diagonals.....	1	2	5	1,899.24*	.839
B. Uniform association.....	2	3	1	212.36*	.094
C. Row effects.....	3	4	3	73.08*	.032
D. Column effects.....	3	5	3	40.33*	.018
E. Row effects:					
(I).....	5	6	3	106.15*	.047
(II).....	5	7	3	96.72*	.043
F. Column effects:					
(I).....	4	6	3	73.40*	.033
(II).....	4	7	3	63.97*	.028
G. Homogeneity:					
(I).....	6	8	3	4.48	.002
(II).....	7	9	3	4.65	.002
H. Constraints on symmetry:					
(I).....	8	10	1	1.93	.001
(II).....	9	10	1	11.53*	.005

* Diagonal fitted exactly or (in the case of models 7 and 9) deleted.

* $P < .05$.

the distances between categories are the same for men and women, as indicated by the equality of the row and column effects.⁴

Expected frequencies under model 8 are in the second column of table 1. Estimates of the parameters are in table 3. The largest status difference is between sales or clerical occupations and blue collar occupations. This is shown by the large difference between b_4 and b_3 . Elsewhere the association due to these status effects is weaker. The differences between b_5 and b_4 and between b_3 and b_2 are actually negative.

Geometry offers a way of interpreting the b 's. Consider the log odds on higher status for one spouse at each level of status for the other spouse:

$$\Phi^{HW}_{ij} = LN(F_{i,j+1}/F_{ij}),$$

and

$$\Phi^{WF}_j = LN(F_{i+1,j}/F_{ij}).$$

Under model 3 (uniform association) and model 5 (column effects), Φ^{HW}_{ij} is a linear function of i , and under models 3 and 4 (row effects), Φ^{WF}_j is a linear function of j . Those linear functions are straight lines from farm to professional-managerial occupations. Under models 6-9, these lines are not straight. For model 8, the b 's are the deviations of categories 2, 3, and 4 from the farm-to-professional-managerial line.

TABLE 3
PARAMETER ESTIMATES FOR HOMOGENEOUS ROW AND COLUMN EFFECT
MODEL (I) FOR HUSBAND-WIFE AND FATHER-SON TABLES
(Additive Form)

Effect by Occupational Category	Husband-Wife Coefficient	SE	Father-Son Coefficient	SE
Uniform effect (b)	-.090	.032	.112	.015
Row or column effect ($b_i; b_j$):				
5. Professional-managerial	0	...	0	...
4. Clerical-sales	.299	.059	.095	.040
3. Blue collar	-.108	.044	-.122	.033
2. Service	-.175	.062	.058	.041
1. Farm	0	...	0	...
Diagonal effect (d_i):				
5. Professional-managerial	1.664	.200	.481	.139
4. Clerical-sales	-.330	.087	.126	.072
3. Blue collar	.812	.074	.325	.038
2. Service	.088	.169	.904	.119
1. Farm	4.694	.343	2.555	.186

NOTE.—The model is: $LN(F_{ij}) = a + a_{1(i)} + a_{2(j)} + b_{ij} + b_{1j} + b_{2i} + d_{ik}$, where $b_1 = b_2 = 0$ and $k = 1$ if $i = j$ and 0 otherwise.

⁴ As Clogg (1982) notes, model II is actually a better scaling model than model I. In that regard, note that model 7 does not improve on model 9 (comparison G II in table 2); thus we fail to reject homogeneity (symmetry) for model II as well as for model I.

Because b_4 is greater than zero and b_8 is less than zero, the log odds for sales and clerical occupations (Φ^{HW}_{4j} and Φ^{WH}_{4i}) lie above the farm-to-professional-managerial line and the corresponding log odds for blue-collar occupations (Φ^{HW}_{8j} and Φ^{WH}_{8i}) lie below the line. Therefore, the biggest increase in the odds on higher status for one spouse occurs with the transition of the other spouse from blue-collar to sales or clerical work. In other words, model 8 predicts the largest increase in expected status at the boundary separating white-collar from blue-collar occupations. Because these parameters pertain to both rows and columns, they affect not only deviations from the farm-to-professional-managerial lines, but also the slopes of those lines. For the associations between husbands' and wives' occupations, the pattern of b 's indicates that the odds on one spouse's having a sales or clerical job relative to a blue-collar job rise more steeply with increasing status of the other spouse than do the odds on being in the higher category of any other pair of adjacent categories. In fact, the odds on service relative to farm and on professional-managerial relative to sales or clerical decline slightly as the other spouse's status increases (when the diagonal effects are partialled out). The conclusion to be drawn from these parameter estimates is that, aside from the process that sorts husbands and wives into similar statuses, the only substantial status boundary is the one between white-collar and blue-collar occupations.

In addition to the status effects, substantial consistency effects contribute to the association between husbands' and wives' occupations, as the substantial diagonal parameters indicate.⁵ The ratio of L^2 for quasi-independence (model 2) to the L^2 for independence (model 1) shows that all but 16.1% of the total association between husbands' and wives' occupations is due to consistency between spouses. Farm couples are most likely to be consistent. Consistency is also strong for professional-managerial occupations and for blue-collar occupations. Sales and clerical occupations display a pattern akin to status disinheritance ($d_4 < 0$). Consistency is insignificant for service occupations; d_2 is less than its standard error.

The joint effects of the diagonal and row-column parameters can be seen in the pattern of the odds ratios for 2×2 subtables formed from adjacent rows and columns of the husband-wife table that are fitted under model 8 (shown in the upper-left-hand panel of table 4). Only the lower triangle of the 4×4 table is shown to emphasize the symmetry of the model. Odds ratios are logged to show clearly the presence of both positive and negative association in the subtables. The largest log-odds ratio

⁵ Diagonal parameters are equivalent to "new mobility ratios" in logarithmic form (Goodman 1969; Hauser 1978, 1979; O. D. Duncan 1979).

TABLE 4

COMPARISON OF LOGGED-ODDS RATIOS FOR SYMMETRICAL HUSBAND-WIFE
AND FATHER-SON TABLES BY MODELS EMPLOYED

CONTRASTS	HUSBAND-WIFE				FATHER-SON			
	1:2	2:3	3:4	4:5	1:2	2:3	3:4	4:5
Homogeneous Effects Model								
1:2.....	.65				.53			
2:3.....	.35	1.21			.11	1.00		
3:4.....	-.32	-.43	.94		-.16	-.18	.98	
4:5.....	-.56	.14	-.29	4.34	.08	.39	-.91	3.69
Quasi-Symmetry Model								
1:2.....	.64				.53			
2:3.....	.36	1.19			.09	1.01		
3:4.....	-.38	-.38	.92		.07	-.34	1.01	
4:5.....	-.39	-.08	-.16	4.25	-.22	.61	-.95	3.73

in the table is that on the diagonal contrasting service and farm occupations. Its size attests to the strength of the farm-nonfarm boundary in these data. The next largest log-odds ratio is at the other great status boundary in the occupational hierarchy—that between white-collar and blue-collar occupations. A boundary between blue-collar and service occupations is also in evidence as is a modest one between upper and lower nonmanual occupations. The other log-odds ratios are not large, indicating weaker association away from the diagonal. Some of the ratios are even less than zero. Although the parameter estimates and log-odds ratios depend on the ordering imposed on the occupational categories,⁶ no permutation of the rows and columns can make all of the log-odds ratios in table 4 positive.

To summarize the findings to this point, an analysis of the associations between husbands' and wives' occupations has shown (1) the association is symmetrical; (2) most of the association is due to status consistency between husbands and wives; (3) the strongest off-diagonal effect occurs at the white-collar/blue-collar juncture; (4) strong status-group boundaries separate farm from nonfarm and manual from nonmanual occupations.

A COMPARISON WITH FATHER-SON MOBILITY

As stated in the introduction, the association between husbands' and wives' occupations can be viewed as one of several indicators of permeability in

⁶ This situation is not quite as serious as it at first seems since L^2 and the fitted frequencies are not affected by the reversal of one pair of rows and columns.

a status hierarchy—the most common indicator being the intergenerational association between fathers' and sons' occupations. In this section these two indicators are compared for the United States in the 1970s.

The third column of table 1 shows the effective counts from the cross classification of father's (or other head's) occupation by son's first occupation using the same classification scheme used for husbands and wives. The data are a special tabulation from the OCG-II supplement to the CPS of March 1973 (Featherman and Hauser 1978) generously provided by Robert M. Hauser.

In comparing the husband-wife and father-son tables, only the symmetrical parts of the associations are compared. Asymmetry is not compared because the relationship between husbands' and wives' occupations is not causal, so it is unclear which spouse's occupation would correspond to father's occupation and which would correspond to son's. Since symmetrical association tables are indifferent to transposition of rows and columns, a one-for-one analogy is not necessary when fitted counts from symmetrical models are compared.⁷ Fitting a symmetrical association model to a clearly asymmetrical table is of dubious value, but two symmetrical models fit both the husband-wife and father-son tables adequately for purposes of this comparison. The two models used for this analysis are the homogeneous row and column effects (I) model preferred for the husband-wife table and the symmetric association (quasi-symmetry) model (Haberman 1979, pp. 490–99). Both models fit the husband-wife table very well (table 2). They also fit the father-son table well, accounting for all but one-half of one percent of the association in the father-son table (table 5).

Parameter estimates for the homogeneous effects model are in table 3, and the log-odds ratios fitted under both models are in table 4. The similarity between the hierarchies revealed by the husband-wife and father-son associations is striking.⁸ The pattern of parameter estimates is similar,

TABLE 5
ANALYSIS OF FIT OF THREE MODELS APPLIED TO FATHER-SON DATA

Models	<i>df</i>	L_m^2	L_m^2/L_1^2
1. Independence	16	5780.03*	1.000
8. Homogeneous row and column effect (I)* . . .	7	30.32*	.005
10. Symmetric association*	6	20.68*	.004

* Diagonal fitted exactly.

* $P < .05$.

⁷ I am indebted to Robert M. Hauser for his comments on this point.

⁸ Statistical tests for differences between tables 1 and 6 are inconclusive. When the homogeneous effects model is the basis for comparison, each diagonal parameter for

though the magnitudes are less for the father-son association. The patterns of log-odds ratios in the four panels of table 4 are essentially the same. The largest entry in each instance is at the farm-nonfarm boundary. The boundary seems to be less permeable for husbands and wives than for fathers and sons. The association at the white-collar/blue-collar boundary is also slightly stronger for husbands and wives than for fathers and sons. Some divergence in the pattern is evident in the last row of each panel (the service-farm comparisons away from the diagonal), but these log-odds ratios are based on very small expected frequencies in the husband-wife table, so they are very sensitive to small changes.

These results support the interpretation of the two associations as indicators of a single underlying status hierarchy. The same boundaries are in evidence, and the status consistency represented by the diagonal is more important than the off-diagonal association in both tables. The boundaries are somewhat more permeable across generations than for cohabiting workers. The explanation most likely lies in the spatial and educational opportunities that separate sons from fathers but not spouses from each other. Many sons enhance their mobility by migrating, but for married couples, migration may enhance one's chances while constraining the other's. More important, the educational system fosters the mobility of sons, but, since for most couples marriage comes when schooling is complete (or nearly so), education is likely to lead to consistency of occupations for spouses.

DISCUSSION

Sociological treatment of the occupational structure presumes the existence of an underlying status dimension along which occupations and their incumbents are ranked. Most of what we know about status has come from analyses of the association between the occupations of fathers and sons. The centrality of the father-son tie stems from the conception of status as a gravitational force that, to the extent to which it operates at all, affects proximate bodies more than it affects distant ones. Until relatively recently, fathers and sons were the most common proximate bodies in the occupational structure. With the increase in two-earner families, a new type of proximity has presented itself—the proximity of spouses. This

husbands and wives is significantly different from the corresponding diagonal parameter for fathers and sons, as indicated by the L^2 for models with and without cross-table constraints on each diagonal parameter. But when the quasi-symmetry model is used, the second (lower nonmanual) and fourth (service) diagonal cells appear to be weighted equally. Allowing the diagonal entries to vary freely while constraining the off-diagonal interactions to be equal across tables significantly worsened fit for both models, indicating some small but significant off-diagonal differences between tables.

paper addresses the traditional questions about status to the association between husbands' and wives' occupations.

The two great status boundaries—those between farm and nonfarm occupations and between manual and nonmanual occupations—appear in the association between husbands' and wives' occupations. The boundaries arise from status effects of two distinct types. The farm-nonfarm boundary reflects the essential social, spatial, and technical constraints that make farming the most common occupation for spouses of farm workers. The farm effect is a status consistency effect operating only on the diagonal of the husband-wife table. On the other hand, the boundary between manual and nonmanual occupations is the result of a general status effect that separates lower nonmanual occupations from blue-collar occupations, no matter what the other spouse's occupation is.

A second important finding is that the association between husbands' and wives' occupations is symmetrical. Whether the reference is to the wife or the husband, the parameter relating one occupational category to another is the same. The symmetry indicates that although the five-category scheme used for this analysis is not an interval scale by any means, the distances between categories are the same for men and women. Some caution must be exercised in generalizing this finding, however. The 5×5 representation of the occupational structure used in this analysis is a highly aggregated image. Men's and women's occupations are highly segregated within the categories used. Further disaggregation may uncover some asymmetry. For now, however, we cannot reject the hypothesis of a common status hierarchy for men and women.

The most important finding is that the process generating the husband-wife table is very similar to the process generating the father-son table. This inference is based on the similarity of parameter estimates for the homogeneous effects model (I) and of odds ratios for subtables of the two larger tables. Odds ratios along the diagonals of the two tables have the same rank ordering. The two largest are at the farm-nonfarm and manual-nonmanual boundaries. The boundaries appear to be somewhat more permeable for sons than for husbands and wives. Off-diagonal associations are much weaker than diagonal associations in both tables.

This research can be refined in several ways. The first priority must be to develop a better model of the differences (and similarities) between the father-son and husband-wife associations. Future research must decide whether the differences observed here are functions of diagonal effects, general effects, or combinations of both types. More detailed categories will help specify similarities and differences as will generation of three-way cross-classifications of the occupations of fathers, sons, and the sons' spouses.

Another important priority for future research is the disaggregation of occupational categories. As noted above, the symmetry between husband's and wife's status effects may not hold up when more categories are used. The number of categories is also important for the estimation of diagonal effects. As the number of categories increases, the diagonal declines in relative importance. Given the ad hoc nature of diagonal effects (O. D. Duncan 1979), diminishing their importance is a desirable goal.

Theoretical issues need to be explored as well. An important issue, beyond the scope of this paper, is the implication of a close association between husbands' and wives' occupations for status inequality among families. Rossi and his associates (Rossi et al. 1974; Sampson and Rossi 1975) have depicted the status of two-earner families as the sum of husband's and wife's characteristics. For simplicity, let us ignore characteristics other than occupational status (scaled so that all relationships are linear and additive). Then family status is given by: $S_f = a + b_1S_h + b_2S_w$, where S is status and f , h , and w are subscripts for family, husband, and wife, respectively. The variance of S_f is: $\text{Var}(S_f) = b_1^2\text{Var}(S_h) + b_2^2\text{Var}(S_w) + 2\text{Cov}(S_h, S_w)$. According to this specification, the variance in family status among two-earner families is a positive function of the association between husband's and wife's status.

Although this analysis has not estimated $\text{Cov}(S_h, S_w)$, it has uncovered a substantial association that is, on the whole, positive. The findings indicate that were an appropriate scaling available $\text{Cov}(S_h, S_w)$ would be shown to be large and positive. The conclusion is that inequality among families is greater than it would be if the association were weak.

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Commentary and Debate

To conserve space for the publication of original contributions to scholarship, the comments in this section must be limited to brief critiques. They are expected to address specific errors or flaws in articles and reviews published in the *AJS*. Comments on articles are not to exceed 1,500 words, those on reviews 750 words. Longer or less narrowly focused critiques should be submitted as articles. Authors of articles and reviews are invited to reply to comments, keeping their replies to the length of the specific comment. The *AJS* does not publish commenters' rebuttals to authors' replies. We reserve the right to reject inappropriate or excessively minor comments.

A SCIENCE OF POLITICS?

Orum ("Is a Science of Politics Possible?" *AJS* 87 [September 1981]: 445-52) tells us that a science of politics is possible. Its objective (and test?) is "to understand that which has happened, and that which may happen, in the world of politics. To understand is to put the pieces of a puzzle together in such a way that we . . . can find the solution satisfying" (p. 445).

Throughout Orum's review essay there are references to comprehending actors and developing a feel for political participants. Orum concludes that a science of politics will show us what makes "political combatants act the way they do and think the thoughts they have," and that such a science will do this "with passion" (p. 452).

It would be interesting to know what Orum has in mind when he invokes "science" as justification of his "understanding." In ordinary usage, science denotes knowledge. Knowledge requires a reliable taxonomy of acts, actors, and situations and the public testing of principles that regularly connect these categories of events. Knowledge gives understanding, of course, but it is not the kind of comprehension that is verified by "feelings," "insights," and other such satisfactions.

A science of politics may be possible, but it will not develop out of the "understanding" that Orum finds satisfying.

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REPLY TO NETTLER

It is difficult to know, in the nonphilosophical sense, where to begin in responding to Nettler's remarks. Reams of materials have been written about the nature of knowing and the nature of a science of politics, not to say a science of society. And even after these many documents, a number of them exceedingly sophisticated and rigorous, we still have not resolved the issue of how to gain substantial and verifiable knowledge about the social world. So, rather than entangle myself in the old controversies, I approached the matter of knowledge in a slightly different fashion—one that may not have been evident to Nettler. I paraphrased the question with which Immanuel Kant had embarked on his critique of pure reason (which was a discourse about how one could produce with apodictic certainty statements of knowledge about such matters as space, time, and number) and I asked how C. Wright Mills, Hannah Arendt, and several other scholars produced verifiable statements of knowledge about the worlds about which they inquired, namely, the world of American politics in the mid-1950s, or the world of anti-Semitism and totalitarianism in Europe through the mid-1940s.

Among social scientists there will be differences of opinion about whether the claims of Mills, or Arendt, or even Dahl about the worlds they sought to explain were genuine claims of knowledge. From my own point of view they were—or, at least, some of them were—because they helped to forecast how later events would appear. Mills's claims about the concentration of power and about the citizenry in the 1950s were followed by events in the 1960s that gave truth to his claims, as were Arendt's characterizations of the historic nature of anti-Semitism in Europe and of the general character of those regimes she described as totalitarian. To verify her pronouncements about totalitarian regimes, all one has to do today, of course, is to follow the news, as best one can, about Poland.

What, then, produced those statements of knowledge which are found in the works of Mills, Arendt, and the others? It is here, evidently, that Nettler seems to find some difficulty with my formulations. While it seems clear to me that Mills and Arendt helped to elucidate the motives and the meanings of actors in the United States and Germany, and their multifarious sources in history, and thus to provide us with the basis for understanding those actors and their actions, Nettler disavows such understanding as a basis for knowing the social world.

It is hard for me to be any clearer, or plainer, about the nature of understanding, and the importance of uncovering motives and meanings among social actors, without undertaking the sort of discourse on methods that I have found to be exceedingly dull and often quite beside the point.

As a few final words to Nettler, let me just say the following. The stuff in the social world that I find important, in the nature of politics but also in the nature of other things, is stuff which is not easily amenable to the standard operationalizations and tests conducted by so many social scientists. "Belief," "motive," "meaning," and "action" are among the qualities that I think social scientists must seek to uncover; the beliefs of people as they conduct their offices, or pursue power, or oppress others are the elusive phenomena that social scientists ought to pursue even if they have frequently failed to do so. These beliefs and motives are historically situated and socially constructed and defined; they are complexly implicated in the very social life we seek to explain. To define them independently of situations—as though they were themselves independent entities, some kind of formal properties of actors and groups—is to destroy analytically the very thing we want to explain. Hence, by my reckoning, to gain knowledge of social life by understanding requires that we approach that world we want to explain both as a whole, a gestalt if you will, and as one that is historically conditioned. This approach, if taken seriously, implies a far different kind of social science than that which many of us do, or have done, but it is one that, I believe, would generate a more reliable, a fuller, and ultimately, in the nonphilosophical sense, a truer explanation of the social world.

At the conclusion of my review essay I asserted that programmatic pronouncements about the nature of social science generally were worthless. I believe the same today, and recommend that Professor Nettler disregard my remarks here as a guide to knowledge in favor of those signal works in the social sciences that have produced such knowledge, such as that by Mills.

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Review Essay: Elizabeth Eisenstein's *The Printing Press as an Agent of Change* and the Structure of Communications Revolutions¹

The Printing Press as an Agent of Change: Communications and Cultural Transformations in Early-Modern Europe. By Elizabeth Eisenstein. 2 vols. New York: Cambridge University Press, 1979. Pp. xxi+450; 341. \$29.50 (vol. 1); \$24.50 (vol. 2); \$49.50 (set).

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We dwell in the very midst of a communications revolution—one which is brighter, louder, and perhaps more penetrative of the human psyche than any that has gone before—but as yet we have no clear idea of what such a revolution is or what it means. Perhaps the very electrified amplitude of the current phenomenon—its brilliance, pervasiveness, and magnitude—screens from us the lineaments of underlying change, preventing us from recognizing what, in general, is the kind of change characteristic of a communications revolution. Without a clear understanding of the kind of change we are talking about, it is impossible to develop a model which explains how this change takes place, a model which does for the history of communication what Thomas Kuhn did for the history of science with *The Structure of Scientific Revolutions*.

Sadly, few see the absence of an explanatory model of communications revolutions as a problem. *The communications revolution*, which conventionally begins with the invention of printing and has proceeded at an ever accelerated rate in the past century, is a historical event, a fact rather than a problem. In George Gordon's *The Communications Revolution* (1977) there is not a line of discussion of what "a" communications revolution is, of how it differs from political, social, scientific, or industrial revolutions. The cultural transformation is most often summarized in terms of technology and its brilliant results: a world in which, in Ben Bagdikian's vision, "all populations will begin to have access to the total knowledge of mankind" (Bagdikian 1971, p. 27) through the media of film, radio, television, computers, print, phonographs, video and audiotape, telephones, etc.

It is important to understand the problems of the phenomenon. If humanity evolves by submitting to governance "by symbolically mediated programs for producing artifacts, organizing social life, or expressing emo-

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tions" (Geertz 1973, p. 48), then the massive augmentation of the means of such symbolic mediation must mean a new stage of human mobilization, a stage which will be a major landmark in the evolution of the species. I do not think Bagdikian is writing science fiction when he proposes that this could "be to politics what nuclear fission was to physical weapons, an increase in power so great that it constitutes a new condition for mankind. The new communications will permit the accumulation of a critical mass of human attention and impulse that up to now has been inconceivable" (Bagdikian 1971, p. 45).

This aspect of cultural transformation often leaves little breath for asking important questions. On *what* will this critical mass of human attention be focused? How will the media shape the direction in which this human impulse is released? How do these new communications alter the very structure of the knowledge which will be so readily accessible to the world's population? Even further, how do these media shape tradition and the stance of audiences before tradition? Do they lend to the "symbolically mediated programs," which have provided different tonalities for the instrumentation of human behavior, implications for political action other than those traditionally provided by the walls of churches, by ritual, and by official proclamations? How do the new means of communication affect an audience's sense of "truth," of "authority," of the very intelligibility of preexisting resources of meaning?

None of these questions can be answered until we know what "a" communications revolution is. Until we have a model which explains how a new communications medium transforms the culture into which it is introduced, the history of communication remains a chronology of invention, something of little use to those puzzling over the destiny of late 20th-century industrial civilization. But with such a model, one that describes the ways in which a new means of communication alters the modes in which human beings fix, appropriate, and disseminate the resources of meaning provided them by their culture, we should be able to see as characteristic those phenomena which only serve now to impress and astonish.

There are two difficulties in generating such a model. First, with the materialistic biases which seem inherent in Western European culture, it is difficult to identify the ways in which "culture" can operate *sui generis* as a cause of historical change. We are accustomed to treating culture as a superstructure shaped and molded by more fundamental transformations in the ways in which men reproduce their material existence, as a structure which ratifies the social arrangements which arise from men's relationships in the material world. And yet, historically, we must contemplate the reversal of this causation. Long before there was any significant alteration in the ways in which Europeans farmed and manufactured, they were "modern" in their morality, their stance toward tradition, their relationship to God and to each other. This is an anomaly within our orthodoxies of historical explanation which draw so much more from Marx than from Weber.

In Marxist terms, it would mean that transformations in the superstructure precede alterations in the "base." Behind this order of priority there might lie a causal relationship. The cultural revolution accomplished in the Renaissance and Reformation might be seen to have "determined" the new ways in which Europeans later worked, manufactured, farmed, and fought. This possibility flies in the face of our dominant ethos of explanation and gives an enhanced certainty and an inherent reality to material things and physical instrumentalities. This is why it is such a relief to find a technology, the printing press, at the root of this cultural revolution, and it is why McLuhan's attempt to describe this revolution in terms of the relation of machines—of printing to the reified mechanics of the human sensorium—struck so many responsive chords.

Yet another difficulty, closely related to the dominance of a materialist ethos, lies in what might be called our democratic biases. Students of the history of communications have always been much more interested in the effect of a new medium on the human material, on the audience, than in its effect on the ways in which men reproduce their culture. Change is significant insofar as great numbers of people are affected by it. Thus the primary significance of print is often seen to lie in the "mass" literacy which it creates, the emergence of a new kind of public and with it a new kind of political process. The "massiveness" of an effect is our gauge of what is historically significant. It is the mass effect of the new medium of print which is so impressive that it is described as revolutionary and, given this bias, dictates that questions of the effect of all mass media on culture are often mutated into issues of social control. Media are treated as instruments of liberation or enslavement which create either an audience that is rational or a mass incapable of independent critical judgment. This concern with the effects of a medium on an audience neglects the middle term, culture—the symbolic realities which impose meaning on life—which is what an audience receives through the communications medium. The democratic bias is a particularly insidious one, for it obscures the cause of radical change in communicational praxis. One can argue that a medium, in and of itself, has no effect on an audience until that medium has altered the ways in which traditional resources of meaning are codified, fixed, and dramatized. It is "meaning" which has the impact on human beings, and the first stage of comprehending the kind of change characteristic of communications revolutions is a matter of understanding how an instrument of communication reshapes the traffic in symbols which imposes meaning upon human experience.

This is a fundamental point, and from it we might draw our definition of a communications revolution. A communications revolution is, at bottom, a radical transformation in the *modes* in which human beings appropriate their symbolic reality. This appropriation is achieved through a variety of channels: literature, drama, music and the arts, history, religion, and through ritual and ideologies of all stripes. If, as Geertz (1973) argues,

there is no such thing as a human nature independent of culture, if the human species is peculiar in that the programs and recipes according to which it organizes social life, expresses emotions, and produces artifacts are external to the organism—matters of culture rather than genetics—then a transformation in the modes through which such programs and recipes are fixed and broadcast must necessarily produce some alteration of human character, some change of “mind.” We cannot think of communication in terms of a simple input-output theory, as an experience in which a particular medium has certain characteristics in and of itself which shape the experience of those who use it. Between the input of, for example, print and the output of behavior stimulated by what a particular person reads lies an entire structure of autonomous cultural functions. To understand the impact of the media on culture, one must look for change in the ways in which a medium modifies intrinsic cultural activities which are essential to the ways in which meaning is reproduced, verified, actualized, by those responsible for the maintenance of particular cultural patterns—priesthoods, men of learning, and communications “elites.” Such an understanding would allow us to divine the direction of change going on about us and to understand why art, popular forms of music, drama, and dance, currently mobilize a greater mass of human “attention and impulse” than politics has ever done.

* * *

Not only does Elizabeth Eisenstein’s summa, *The Printing Press as an Agent of Change*, offer us a classically “thick” description of how the introduction of a new medium reorients European culture, it also redirects our attention to the issue of how cultural transformations can be produced by changes in the means of communication. From her description of the relationship between print and the intellectual revolution of the Renaissance and Reformation, we can abstract some of the elements of a communications revolution and apply them to a contemporary context, identifying the differences and similarities in the patterns of transformation.

The main focus of her book is not on the ways in which print creates a new audience for books but on the ways in which it alters the shape of the “commonwealth of learning” and establishes a new division of labor among its citizens. Her general thesis is that the most significant effects of print lie not in the ways in which it transmits information but in the way in which it “fixes” and secures tradition. Much of what is new in the Renaissance and Reformation is a product of the ability of typographical culture to secure what is old. This frees intellectual elites from the constant performance of essential cultural patterns, allowing them to reflect on those patterns, organize them, make conscious the values implicit in them. This shift of cultural effort from the physical replication of the symbolic reality to its conscious manipulation and verification results in the “cumulative” cognitive advance which distinguishes European culture from others as

something "modern," progressive, and productive of economic, political, and social innovation.

This abstract does not, however, suggest the multiplicity of linkages which Eisenstein demonstrates, deduces, or infers between printing and the Renaissance. In order to understand these linkages, one must understand what she means by "typographical fixity"—for this is the source and root of a much broader cultural reorientation. In the first instance it means that print *physically* secures the received body of texts and images on which authoritative definitions of the meaning of things traditionally rested. Manuscripts have a precarious existence, vulnerable to natural and man-made disasters, dependent on shifting intellectual fashions and the availability of a corps of scribes. Moreover, print physically secures the corpus of the written tradition by a means the very opposite of that formerly in use. It is not by secrecy but by publicity, not by its limitation to a small band of adepts but by its broadest diffusion, that the security and potency of knowledge are assured.²

The security which print lends to the received tradition produces a profound transformation in the attitudes of those responsible for maintaining that tradition. This new stance is best seen in the historical consciousness that is such a novel feature of the Renaissance. The Renaissance was essentially a revival of classical learning which differed from previous such revivals in that it was "total and permanent" rather than partial and transitory. Renaissance humanists operated with a corpus of models which were at once secure and "removed from them" by a "fixed distance" of a thousand years—the Middle Ages. As Erwin Panovsky originally stated, "In the Italian Renaissance the classical past began to be looked at from a fixed distance. . . . This distance prohibited direct contact . . . but permitted a total and rationalized view" (quoted in Eisenstein, p. 118) of the past. The fixed distance which sacralizes ancient texts would have been impossible, Eisenstein argues, without typographical fixity. "That a total rationalized view of any past civilization could be developed before the output of uniform reference guides and gazetteers seems implausible to me. How could the entire classical past be viewed from a 'fixed distance' until a permanent temporal location had been found for antique objects, place names, personages and events? The capacity to see the past in this way could not be obtained by the new optical effects devised by Renaissance artists. It required the rearrangement of documents and artifacts" (Eisenstein, p. 186).

The puzzle of the Renaissance lies in the connections between the two

² This much was understood by Thomas Jefferson, who, in attempting to collect the laws of the state of Virginia for the first time, found that many single copies and priceless precedents had gone to satisfy the hygienic needs of generations of clerks. This leads him to a higher consideration: "How many precious works of antiquity were lost while they existed only in manuscript? Has there ever been one lost since the art of printing has rendered it practicable to multiply and disperse copies? This leads us then to the only means of preserving those remains of our laws not under consideration, that is, a multiplication of copies" (quoted in Eisenstein, pp. 115-16).

processes noted by Panovsky and related by Eisenstein to the invention of printing. On the one hand, with print, scholars can more easily fill up time, establish chronologies, date documents, fix the "before" and "after" on which historical logic depends. On the other hand, one sees in the Renaissance the "rebirth" of classical forms, the establishment of classical models, the evaluation of this record in terms of privileged styles and formulae which are paradoxically "dead," finished, and thus subject to "revival." They are viewed from a "fixed distance," "separated in time" from the present by the span of a thousand years. The newly established classical norms are those which are thought to be worthy of imitation in the present by artists, humanists of all stripes, who seek to surpass these models. They thus dictate the conditions of "creativity." The problem of the Renaissance lies in how the establishment of a "fixed record," a chronology, led to a new kind of periodization (ancient, medieval, and modern) which served to codify the most sacred values of the literate intelligentsia, allowing them to reproduce their culture, to recreate "meaning" in a more rational and original way.

It is important to understand that the Renaissance distinction between medieval and ancient was no mere distinction of time; it was also a distinction of value clearly present in the difference between the price paid for the "antique" and that of the merely old. The antique was removed in time, and this removal by a thousand years might be regarded as a kind of temporal sacralization, for sacralization is nothing more than a process of "setting apart," a circumscription of the object destined for veneration by prohibitions and taboos which prevent contact. It is thus that certain architectural "orders," rhetorical formulae, compositional rules are removed from their normal cultural matrix, becoming "holy" and distinct from all that is profane. At the same time, the object thus sacralized becomes the nodal point of rules, ceremonies, recreations which celebrate its special status. The sacred, for Renaissance men, is subsumed in the classical form of art, language, and religiosity. The models thus distinguished are the end product of a process of discrimination and valuation—one which is fixed not in liturgy but in "history."⁸ History for the Renaissance humanist was neither the pure chronicle of what happened nor the explanation of how the present world came to be what it is for moderns, a story. Instead, it was first a mode of distinguishing "figure" from "ground" in the symbolic universe inherited from the past and offering up the figures as patterns for the rational governing of behavior in the present. Vasari, for example, illustrates this in noting that the activity of historians consists precisely in this process

⁸ Machiavelli reflects this understanding of classical models when he suggests that these models are excellent patterns for imitation precisely because they are "removed" from the present and thus living men have no "interest in distorting" them to their own political, social, or economic advantage. In this sense, they are objective, and the vehicle of rational thought.

of creating a gallery of figures—"good, better and best"—useful in forwarding the enterprises of living men (Vasari 1970, p. 83).

Print did not itself create that classical revival which is identical with the Italian Renaissance; nor did it produce Italian humanism. All of the elements of humanism—the emphasis on classical Latin style, the elevation of rhetoric above scholastic logic, the desire to emulate the ancients, the interest in language and non-Christian cultures—were present before print. But print institutionalized this classical revival, fixing it and making it a Europe-wide phenomenon. The most important effect of print, however, was that it changed the very conditions of intellection. The classical world and ancient documents, once the prized possessions of libraries and archives, became the primary instrument of a more public rationality. The inscribed record became a generally available means by which learned men could more accurately perceive historical differences of style and substance. It is in this that one sees the broadest ramifications of the communications revolution initiated by print. Europeans, with a symbolic reality coded into generally available texts and organized as history, were able to perceive change and even value it.⁴

The structural features of oral communication which serve to obscure the details of a tradition are diminished by writing and utterly effaced by print. The Renaissance is the point of origin not of history but of historical reason, the beginning of a process in which contemporaries establish their identities by arguing with, against, but always in relation to a concrete textual record. Their sense of their own dignity comes increasingly from the extent to which they feel that they surpass preestablished forms, not the extent to which they recapitulate these forms. The very idea of progress in the arts and sciences is inconceivable without the existence of the kind of cultural fixity established by print. Now what is new is *consciously* perceived against the background of an objective record which exists independent of living memory and, with print, independent of any decision about what is to be copied or reproduced. A printed record of the past has an enhanced and very particular and immutable reality, and its relationship to the present as an authorizing reality must be established by logic and reason, consciously and strenuously. The past precedent is stretched to accommodate a present circumstance through precisely argued analogies. In that "rational

⁴ The common assumption that traditional and modern cultures offer a contrast between cultures which are stable and those which change is perhaps rooted not in reality but in the perceptions imposed by the modes of cultural reproduction. The inability to distinguish between the novel and the traditional in the absence of any living memory produces what Jack Goody calls the "presentist" bias of oral cultures (Goody and Watt 1975, p. 95). Genealogical tables, such as those we find in Genesis, are constructed in order to legitimize the relationships and priorities which exist between contemporary lineages and families. They legislate degrees of status in terms of origins. It is precisely this structural amnesia of oral cultures which makes them so flexible, so adaptive to circumstance, while maintaining the unassailable fiction of permanence and continuity.

distance" between the most sacred of classical models and contemporary circumstance, artists, theologians, and scientists now have room to play with their most basic cultural forms. The fixity established for the textual tradition by print thus has significant consequences for the ability of communicators to distinguish between the reproduction of their culture and those alterations of the basic motifs and schemata which constitute a personal addition, an "original" contribution to that culture. One can see how scholars, artists, and scientists increasingly take their dignity not from their ability to reproduce the old but from the ways in which they introduce new articulations of meaning within the established forms. The shift of effort from the replication to the codification of cultural patterns is reflected in the reversal of meaning undergone by the term "original." In its old meaning it meant closest to the origin of things, to the initial creation of the cosmos. In the first truly typographical culture it increasingly meant "novel," a break with precedent.

Eisenstein's work gives us new insight into the traditional problem of how a shift of cultural *function* translates into a change in cultural *structure*. This is particularly evident when one compares the structure of knowledge which obtains before print with that founded on typography. Her argument is that print creates a situation in which certain essential cultural functions—preeminently the preservation of the symbolic reality—are mechanized. This, in turn, permits the development of a certain redirection of effort, a new division of cultural labor, and the germination of a new "ethos" of creativity and verification. "Views which were shaped by the need to preserve data from corruption were incongruous with massproduced objects sold on the open market" (Eisenstein, p. 274). She notes a breakdown in the structure which preserved knowledge, a kind of "decorporatization" of knowledge. Print acted as a solvent on those institutions which preserved and handed down the wisdom of the ancients, simply because it secured the survival of texts by their multiplication and broad distribution. As Panovsky has noted, the Renaissance was a period of "Decompartentalization: a period which broke down the barriers that had kept things in order—but also apart—during the Middle Ages" (quoted in Eisenstein, p. 270). The medieval tendency to compartmentalize, corporatize, and ritualize knowledge was not something rooted in some "medieval mind" so much as a technique of handling information which was necessarily functional, given scribal modes of cultural reproduction. Knowledge was packaged as "mystery," with access to it controlled by and restricted to those who had been initiated into its secrets. "Many forms of knowledge had to be esoteric during the age of scribes if they were to survive at all. Quite apart from issues associated with religious orthodoxy and heterodoxy; closed systems, secretive attitudes and even mental barriers served important social functions. To be preserved intact, techniques had to be entrusted to a select group of initiates who were instructed not only in special skills but also in the 'mysteries' associated with them. Special symbols, rituals and incan-

tations performed the necessary function of organizing data, laying out schedules and preserving techniques in easily memorized forms" (Eisenstein, p. 270). In the early stages of print, however, trade secrets, arcanæ, carefully hoarded mysteries, and unwritten recipes went to press.⁵

Beginning with print we have the development of quite a different structure of knowledge, one which is visualized in the image of "transmission and flow." With a method of preservation which depends not on the limitation of knowledge to fully initiated men of learning but on its broadest possible dissemination, knowledge can be regarded as something which accumulates validity as it passes between a generalized set of writers and an equally generalized corpus of readers. One of the most significant effects of print, Eisenstein suggests, was that it set up a medium of communication in which "feedback" was more possible and frequent. A printed book, unlike a handwritten manuscript, was a standardized product, the same in its thousands of copies. It was possible for publishers to solicit corrections and contributions from readers who, from their own experience, would send back a report—and this was common practice. Within this structure, it was logical to see truth as something which accumulated in the transactions among trained observers, experimenters, travelers, and scanners of the heavens. "The advantages of issuing identical images bearing identical labels to scattered observers who could feed back information to publishers enabled astronomers, geographers, botanists and zoologists to expand data pools far beyond all previous limits. . . . The closed world of the ancients was opened, vast expanses of space (and later of time) previously associated with divine mysteries became subject to human calculation and exploration. The same cumulative cognitive advance which excited cosmological speculation also led to new concepts of knowledge. The closed sphere or single corpus, passed down from generation to generation, was replaced by an open-ended investigatory process pressing against ever advancing frontiers" (Eisenstein, p. 687). It is clear that the kind of data collection which pro-

⁵ One can see the features of the scribal organization of knowledge most clearly in the context of a purely oral culture such as that described by Frederik Barth (1975). Barth suggests that the Baktaman view of the world could be described in terms of an "epistemology of secrecy." Secrecy sanctifies information and preserves its value to the point where the "value of information" seems to be regarded as inversely proportional to how many share it. Secrecy is regarded as the means for "the proper handling of knowledge, its limitation, prohibition from association with contrary substances, its refinement and concentration. . . . Conversely, knowledge may be dangerous . . . and must be controlled and contained" (Barth 1975, p. 218). The very organization of knowledge as a series of Chinese boxes reflects these premises. Not just secrecy but deceit—deliberate lies and obfuscations introduced at one level of initiation so as to obscure the secrets to be revealed at a higher level—is regarded as proper and expected. When one has gone through seven degrees of initiation, although few actually succeed in negotiating the entire structure, one must necessarily become a cynic about the possibility of attaining any ultimate truth. All one knows is just what one's fathers learned "before they died." This structure of knowledge, articulated in rituals of initiation, preserved at each level by deceit and secrecy, generates the ethos peculiar to oral cultures.

vided the ethos of exploration for voyagers and travelers, wandering naturalists and collectors of all kinds of information about the world would have been inconceivable without the means of integrating their newfound information into existing data pools. This means consisted of uniform maps, increasingly accurate guidebooks and compendia, tables and gazetteers. Europeans became collectors of information *par excellence*, and the "information explosion" which they initiated burst old corporate structures designed more for the preservation than for the augmentation of knowledge. This meant, too, the rapid expansion of what we would now call an intelligentsia to include not just professional and certified adepts but also learned and leisured gentlemen of scientific, antiquarian, or literary bent. They, too, could acquire some small permanence, a whiff of immortality, if they added something—however small—to the storehouse of knowledge, or corrected a long-held error. The enormous enthusiasm for science, the sense that now *all* could be known, rested on the realization that, with print, knowledge was an accumulating rather than a dissipating resource.

The difficulties which stand in the way of understanding the radical character of the communications revolution initiated by print—what I have called the materialistic and democratic biases—are themselves a product of this new, open-ended structure of knowledge. Armies of scribes were replaced by armies of readers who are not so much passive receivers of already packaged mysteries as individuals engaged continuously in matching the received symbolic realities against the world they experience. It is this audience which constitutes a force of change in relation to the existing cultural patterns which shape the most fundamental expectations about the nature and meaning of the world. The structure of knowledge, the "commonwealth of learning" founded upon print, is one in which truth is conceived as something that attaches to opinion, observation, hypothesis, fantasy, and fiction as it flows through the neutral and impersonal channels connecting producers and receivers of the symbolic reality. It is in this that it acquires the standing of a natural or social truth. No longer is the symbolic reality of culture conceived to be an unalterable pattern vouchsafed to a few, continually and painfully recovered from incomprehensibility; it is seen instead as a process of continual ratification in the lived experience of men. Truth, now, can be seen as the result of a poll, a questionnaire, a series of questions which men of learning ask of an audience of "responders"—human or natural.

This open-ended structure of knowledge sets out the premises within which our modern communicational realities are generated. Even the hardware, the technology, originates within a model of communication abstracted from the world of print. As Raymond Williams points out, "Unlike all previous communications technologies, radio and television were systems primarily devised for transmission and reception as abstract processes, with little or no definition of content" (Williams 1973, p. 25). One might make a similar point about the intellectual environment within which the

invention of photography and cinematography seemed a feasible enterprise, for it is questionable whether the development of a mechanical means of fixing the observed reality would have been thought to be worthwhile outside the traditions of European naturalism, traditions which endow the observed reality with such power to ratify or falsify received conceptual models. A camera would tell nothing to someone who sees truth as veiled, hidden within mysteries, while it would be a functional instrument for those who have a secure grasp of the "distance" between symbolic reality and the true, but insignificant, nature.

The effect of print on the ethos of those charged with the preservation and verification of the symbolic reality was to raise "objectivity" to the status of a new perceptual ideal. Men who lived and thought within the profusion and complexity of "things to think with" provided by the printing press found it increasingly necessary to distance themselves from this profusion, to introduce conscious distinctions of value within it, between "magic" and "knowledge," "ancient" and "merely old" wisdom, "ways to go to heaven" and "ways that the heavens go." This distance was structured by new scientific and scholarly disciplines, routines of evaluation which were designed to remove the observer from all those unexamined cultural assumptions, customs, habits of mind that might cloud his vision. Francis Bacon was perhaps the first to provide a clear statement of how the ways of knowing proper to the scientist differed from "common" understanding. The scientist, for Bacon, was no longer one who combs the texts for the nuggets of truth planted there by the ancients, nuggets corrupted in the years of passage—this was "word magic." Too, the scientist is the individual who has identified and limited the effects of all of those things we call culture but which Bacon called "idols": personal taste, common language, the authority of the ancients, religion. Bacon insisted that the genuine knower was he who had regained an innocence of eye at least comparable to that enjoyed before the fall of human intelligence into a laborious decrepitude. He was the man who had liberated himself from definition by his culture. (This is an astounding ideal, and yet, again and again, we find it to be the essence of the way in which Renaissance intellectuals perceived themselves and an essential ingredient to our basic concept of the Renaissance. Perhaps the most famous, oft-quoted, and misunderstood statement of this ideal is that written by Burckhardt: "In the Middle Ages both sides of human consciousness—that which was turned within as that which was turned without—lay dreaming or half-awake under a common veil. The veil was woven of faith, illusion and childish prepossession. . . . Man was conscious of himself only as a member of a race, people, party, family or corporation, only through some general category. In Italy, the veil first melted into air; an *objective* treatment and consideration of the state and of all things of this world became possible. The subjective side at the same time asserted itself. . . . Man became a spiritual individual" (Burckhardt 1937, p. 70).

For modern historians, trained in the faith of cultural relativism, this passage is calculated to set their teeth on edge, for it raises the whole specter of the superiority of European culture over the rest. To Burckhardt, the Renaissance was the point at which individuals were freed from domination and definition by their social and cultural matrix. It would now be logical to assert that the perceptions of an individual, freed from all his memberships, have a "universality," an objective truth, not accessible to those enmeshed in the toils of inherited identities. These perceptions could now be integrated back into the corpus of human knowledge as science, philosophy, "truth"—forms of knowing innately superior to "faith, illusion and childish prepossession." The Renaissance was a sudden awakening of a "human" consciousness, just as Italy was privileged to be the site of this new stage in the unfolding of the world spirit.

This set of claims, essential to Europeans' sense of their own superiority, a superiority now only scantily clad in the trappings of the "modernization problem," is unacceptable for those who follow Geertz in believing that to be liberated from one's culture is to be cast into the domains of meaninglessness: "Undirected by cultural patterns—organized systems of significant symbols—man's behavior would be virtually ungovernable, a mere chaos of pointless acts and exploding emotions, his experience virtually shapeless. Culture, the accumulated totality of such patterns, is not an ornament of human existence but the principal basis of its specificity—an essential condition of it" (Geertz 1973, p. 46). But with the clarifications offered by Eisenstein of the kind of cultural change which underlay the Renaissance, we can see that Burckhardt's characterization of the Renaissance and Geertz's position are not necessarily contradictory. It is possible for individuals to experience their cultural patterns and "organized systems of significant symbols" as something outside of them, something objective, once the textualized definitions of these patterns are secured—as they were for Europeans by the invention of printing. It is possible to set aside, as a given, all those inherited models which no longer rely on the constant performance of the living for their efficacy, and to perceive them from a "rational" distance once they are eternally graven in the "record." They no longer have the immediacy which they hold for those who are condemned to their constant, physical replication. Similarly, as Eisenstein so perceptively points out, individualism is a socially prescribed faith, fostered by the profusion of standardized types and roles so perfectly illustrated and specified in thousands of editions of printed books on the manners, style, and behavior proper to the gentleman, the courtier, the father, mother, son and daughter, the clergyman. "Concepts pertaining to uniformity and to diversity—to the typical and the unique—are interdependent, they represent two sides of the same coin. In this regard one might consider the emergence of a new sense of individualism as a by-product of the new forms of standardization. The more standardized the type, indeed, the more compelling the sense of an idiosyncratic self" (Eisenstein, p. 84).

Print, in the 16th century, constituted a means by which Europeans could reproduce their symbolic reality in "exactly repeatable" pictorial and textual form. All of the models for thinking, the images of identity, symbols of sacred and profane—all that constitutes the cultural realities codifiable in inscribed forms—went to press and became a menu of templates directly available to more individuals. This profusion of symbols, where it was adequate to experience, ensured a sense of congruence between ideas and events. But where it was not, where the cultural patterns disaccorded with life as lived, here was information of a novel kind—information that revealed even more about the actual identity of things and individuals than the concordances. Individualism, the definition of the normal person as someone who enjoys a degree of political autonomy over his own person and property and who relates to others through the formalities of personal contract, is implicit in feudal social relations, at least at the highest social levels. Thus print and the Renaissance did not create individualism, as many have claimed. But they did set up more effectively a range of secure models, images, and symbols clarifying and broadcasting the essential traits of the type, making it a conscious and manipulable pattern of characteristics for those outside the range of courts or the circuit of the yearly peregrinations of the nobility, providing them with a uniform language of identity.

* * *

I have tried to do justice to Eisenstein's argument, even at the cost of many of the qualifications, shadings, and cautions she introduces into it, cautions which make *The Printing Press as an Agent of Change* into a series of often brilliant but sometimes halting suggestions. But a historical thesis can never have the clarity possible in those disciplines which can manufacture the requisite data, for there is always the inherent intractability of the record to deal with, a function of the "fixity" of point. Here I would like to set the facts aside for a moment in order to generalize about what happens to a culture in the throes of a communications revolution. At the very least, such generalizations should tell us what we might look for in our own cultural developments; at the most, they will allow us to identify the areas of darkness against the vast obscurity of the subject.

We might begin by arguing that what print did for scribal culture—securing it and creating the conditions of a phase of "classicization," the clarification of essential cultural patterns—the new media are doing for contemporary industrial civilization. The differences between the revolution of print and our own communications revolution are sufficiently obvious. Print served the needs of a highly literate cultural elite, providing the institutional basis for an intelligentsia which created its own values, canons of truth; its own ideology was, willy-nilly, codified into the very information which eventually reached the literate "masses." Indeed, literacy became a passport to social dignity. The new media, including phonographs, radios,

and television, have removed this limitation and made accessible to the non- or semiliterate an acoustical and visual range of our symbolic universe which is not easily represented in more traditional forms. The social and physiological impact of the new media is much broader than that of print, but still it is possible to detect similarities in the pattern of this impact.

With Eisenstein's clarification of what a "renaissance" is—the epuration of essential cultural "figures," and their separation from "ground"—it is possible to address the question of how a new medium of communication can transform the modes in which we appropriate our symbolic reality. The Renaissance effected a standardization of old European culture articulating the most privileged styles and symbols of this culture in time, as history. But in standardizing essential cultural figures, styles, and images of identity, 16th-century artists, scientists, editors, and literary humanists provided a means by which the *differences* between these models and life as lived, nature as experienced, could be detected and read into the written record. This new and clearer sense of the difference between symbolic and nonsymbolic realities made pervasive a new sense of the "realities" that existed "outside" or "below" inherited traditions. Change in the essential cultural modalities began to be regarded as normal insofar as these discordancies were collected and used to correct the received schemata. The ease with which we perceive change is dependent on the degree to which we receive "standardized and duplicable" images against which the unstandardized and "meaningless" flux of experience can be measured, and this is the achievement of print and, even more, of the "new" communications technologies.⁶

It is not hard to understand why such massive efforts to clarify essential cultural figures and their contrast to ground have been precipitated by the acquisition of a new medium. With the advent of the printing press, all that scribal culture had succeeded in preserving or recovering—magical formulae as well as scientific hypotheses, courtly fantasies and philosophical treatises—was fixed in print. The profusion of images, the ubiquity of the

⁶ The process of classicization might be understood in terms of the process which gestalt therapists regard as essential to perception, the process of distinguishing "ground" from "figure." Often this distinction depends on the repression of irrelevant signals so that the meaningful may be left uncluttered and exposed, readily identifiable and immediately efficacious in rendering experience meaningful. "The construction of *Gestalten* would seem to depend upon something like inhibition—a partial negation of certain impulses—which permits the perceiver to attend to those matters which he perceives as 'figures'" (Bateson 1951, p. 174). This repression of certain impulses, their negation as irrelevant, occurs during renaissances as the creation of "distance." It is a process which has been historically contemporaneous with the acquisition of a new medium of communication, a new and more secure means of codifying, securing, and disseminating essential cultural patterns. The areas of "ground" or "noise" are the result of the formation of conscious criteria about what is "good," "better," or "best." What is excised from these categories comes to inhabit a kind of cultural background. In the Renaissance, a "Middle Ages" both functions as a temporal repository of the irrelevant and establishes a "rational distance" between the created models and those who use them in the recreation of culture.

printed word, could easily be perceived as disorder, an embarrassment of symbolic riches, by those whose job was to maintain the intelligibility of received traditions. Thus the "medieval world picture" was much more familiar to Elizabethans than it was to medieval men and women. But in this flood of things "to think with," it becomes difficult to tell the difference between the true, the probable, the unlikely, and the impossible. The development of electronic broadcasting media, film, phonographs, and tapes has created a similar overabundance of information. The consumer of meaning is subjected to a barrage of advertisements, information, entertainment, sound, and image which makes it difficult to maintain essential cultural distinctions—such as those between violence and justice, love and sex, the "good" and the "best." To survive this profusion of symbolic resources initiated by new media it is essential to develop new reading, viewing, and listening habits that involve suspending belief, engaging in automatic low-grade skepticism, or developing new techniques of falsifying information and evaluating fictions. But the preeminent technique of reestablishing clear meanings is the delineation of essential cultural figures and their separation from irrelevant noise, in other words, the process of classicization.

Unquestionably this is a process which occurs in every culture, continuously. But with print, and more so with the electronic media, this process becomes conscious. In oral cultures the process by which items of meaning—concrete symbols—are selected for explication and inculcated into the memories of the living is one governed by the cycles of growth, initiation, ritual, and ceremony. The scheduling of education is governed by the more fundamental rhythms of the life cycle which are essentially moments in the life of the society. Print both individualizes this process and makes it necessary for the learner to become conscious of how he learns, how he selects ideas, images, and obligatory metaphors to use in living a "meaningful" existence.

Something else is new with print. The neglected class of things, those which become cultural "ground," does not disappear as it does in oral and scribal conditions of cultural reproduction. The fixity of print ensures that unpopular styles, impurities, and corruptions introduced into the textual tradition, all unorthodoxies and barbarisms, will remain available as matters of the written record. These materials can be made conscious, are subject to retrieval and revival, and as such can provide the materials for the fashioning of new cultural *gestalts* and sources of identity. The Middle Ages, shaped by Renaissance humanists as a period of time in which all that was merely old might be classified, became for 19th-century critics of industrial society a valuable resource for counterstyles, antiindustrial images of community and the person. If the past becomes a burden for those who are condemned to repeat it in lecture halls and books, it also becomes an invaluable resource of conscious change and adaptation to new social and economic circumstance. The continued presence of the negated class of things transforms the conditions of creativity in typographical culture; it does so even more radically in a culture in which the electronic media

constitute a primary means whereby we fix and appropriate our symbolic realities. This much was understood by someone like Cecil Taylor, who admitted that when he was at a loss for a musical idea, he often had recourse to a "sound," a fragment, from the neglected corpus of Duke Ellington's recorded work. Musical styles are fixed on wax much more accurately than they are by means of written notation. In these circumstances, creativity often appears in the performance, the interpretation of "classical" styles, or in pursuing neglected hints in the work of the "masters," major creators of popular styles. Popular styles, instead of consisting of traditions which bind the words and thoughts of those who reproduce them, no longer define the identities of performers; they now constitute a spectrum of interpretative possibilities, a range of colors of a palette, a metalanguage.

New media are often considered revolutionary insofar as they provide a new and more effective means of standardizing cultural patterns, but it is important to pursue Eisenstein's suggestion that such standardization, uniformity, and simplification become the condition of individuation, originality, and the unexpected complication of traditional schemata. It is in this sense that the securing and emendation of tradition can be considered a necessary condition for cultural change and one that shapes the pattern of this change in a direction sharply divergent from tradition. All the communications revolutions of historical record—the acquisition of the phonetic alphabet by the Greeks, of print by the first "modern" Europeans, of the new electronic media by 20th-century industrialized society—have raised fundamental questions about the relationship between style and identity. For Plato, the way in which style was inculcated by the imitation of the oral poets through *musica*—by which was meant all those conscious and unconscious means by which the movements of individuals are coordinated into social and cultural patterns (history, tragedy, comedy, music proper, poetry, mathematics, astronomy, religion, myth)—produced a protean identity which was inimical to social and political order. This order required a massive emendation of tradition, the determination of what were rational and what irrational roles. Clearly, the fact that writing could encode and record the ways of nonliteracy and make this record available for imitation everywhere lay behind Plato's attempt to purify and rationalize public behavior. The reform of manners, the purification of styles, the eschewing of all "excess," and the fashioning of new and more rigid social disciplines were all a part of the various puritanisms which were such a prominent feature of the 17th-century counter-Renaissance. This, too, can be seen as a response to the luxuriant imagery and symbolisms so characteristic of the Renaissance.

These parallels can be pursued into our own age. A concern for the body, holistic health movements, intense concern for the integrity and purity of the self are logical, even necessary, responses to the proliferation of standardized images of the symbolic body in motion. From the profusion of popular styles, "pure" and "classical" forms are extracted, codified, and presented as imitable by those who want to establish an individuality vis-

à-vis an omnipresent symbolic reality. It is thus that folk, jazz, blues, gospel, tex-mex, and country and western become "classical" styles, vehicles of a pure cultural identity and artistic creativity for those advancing the schemata of cultural reproduction beyond the common levels of performance. It is through such "classical" forms that the reproduction of culture becomes an increasingly conscious, artful, and exacting enterprise, engaging a broad range of specialties and increasingly exact division of labor.

In conclusion, Eistenstein's work leaves the reader with a clearer notion of the research that can be done on contemporary issues, or at least with a sense of the questions which should be confronted in "recommendations for further research."⁷

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⁷ For example, (1) How do the electronic media fix aspects of our industrial culture and allow new forms of cultural self-consciousness to appear in the very reproduction of culture? How are all of those engaged in the reproduction of meaning—musicians, dramatists, producers, and showmen of all stripes—benefited in their work by a new degree of cultural fixity? (2) What are the signs of a renaissance in our own cultural situation, in the ways in which we treat styles? One can see signs of a change in the very processes that underlie "recreativity." Musicians like Ry Cooder, the Pointer Sisters, Manhattan Transfer, Leon Redbone, and a few others treat styles as manipulable quantities in a creative process. They are able to duplicate perfectly the "pure" classical forms and yet mix these in a production which has aesthetic unity—at their best. In what ways do the media function to strengthen this kind of cultural "deutero-learning"? Or are the new media initiating a new structure of knowledge *neither* ritualistic nor adequately representable in the image of "transmission"? (3) How do the new media, and the kind of knowledge (modes of symbolic appropriation) native to them, also produce a new kind of audience and new levels of perceptivity within it?

Book Reviews

Vertical Classifications: A Study in Structuralism and the Sociology of Knowledge. By Barry Schwartz. Chicago: University of Chicago Press, 1981. Pp. ix+243. \$19.00.

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When I had the good fortune to read an early draft of *Vertical Classifications* some time ago, I felt that this book was one of the most significant contributions to the sociology of knowledge made in the past few decades. I am now happy to be able to say that the final version is even more impressive. This is a path-breaking book.

Barry Schwartz's immediate purpose in this work is to explain human beings' cross-cultural propensity to use spatial metaphors like high and low, up and down, above and below to represent social inequalities of various kinds. The exploration and explanation of the universality of vertical classifications and of the preponderant linking of cognitive with moral dimensions—so that what is above usually is judged to be superior and what is below is judged to be inferior—serve Schwartz as springboards to reopen long-neglected inquiries into the linkages between mind and society, between social categories and symbolic representations.

As will be remembered, Durkheim said that categories of thought and logical classifications were not only transmitted by society but created by it. Society, he argued, is decisive in the genesis of thought by furnishing the concepts of which that thought is made. Lévi-Strauss objected to this Durkheimian view that symbolic thought is so fundamental to societal living that sociology, far from explaining the genesis of symbolism, has to take it for granted. Or, as Schwartz puts it: "Durkheim's position is one of circularity: the social relationships to which he appealed for explanation of primitive classification are themselves classifications and so presume what they are supposed to explain" (p. 170). But Lévi-Strauss's explanation, that all systems of classifications are rooted ultimately in the structure of the mind which has inherent constraints causing it to think in terms of binary oppositions, seems equally unsatisfactory to Schwartz. Even if this theory might account for cognitive discriminations, it cannot account for their linkages to moral dimensions. Why should that which is perceived as taller or "above" be judged to be morally superior and that which is smaller or "below" be equated with moral inferiority? Why are the "up" and the "good" correlated? Why does *ethos* largely parallel *eidos*?

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Building not only on clues provided by Durkheim and Lévi-Strauss but also on strands of thinking in modern British anthropology, psychoanalysis, experimental social psychology, and philosophical inquiries, Schwartz develops his own explanatory scheme. Vertical classifications, he argues, conform indeed to Durkheim's classical model of emergent collective representations, such as his distinction between the sacred and the profane, but they are ultimately rooted in a universal personal experience, that of the relation of the strong, superior, tall, powerful, and authoritative parent to the weak, powerless, small, and inferior child. By articulating personal experience rooted in childhood, the vertical code is transferred to the adult world and so becomes a social code that can be used as a universal language with both cognitive and moral meanings. As Schwartz says, "The concomitant invariance of vertically-coded cognitive, behavioral, and affective categories is an expression of their common relation to an underlying archetype of vertical opposition. Such an archetype emerges as a by-product of the universal experience of growing up" (p. 134). Or, to put it differently, our encounter with the socially powerful and with social authority are prefigured by and grounded in our experiences with authority in childhood.

Schwartz accepts Lévi-Strauss's emphasis on the prevalence of binary thinking and Durkheim's crucial bipolar distinctions between the sacred and the profane but roots such distinctions, at least when it comes to vertical classifications, to regularities in culture that are based ultimately on the interactive relationships between parents and children. The "natural" disparities between parents and children are appropriated by society to create the moral and group boundaries that structure social life. Schwartz agrees with Durkheim and the neo-Durkheimians that symbolic systems are always socially constituted but argues that, in order to know how they are so constituted, mere reflection theories of knowledge and moral discrimination are not enough. These insights have to be supplemented by reference to the primal interactive structure of the social experience of childhood.

Schwartz is aware that in contradistinction to the binary distinctions between the inferior left hand and the superior right hand which have been analyzed by the French sociologist Robert Hertz and the British anthropologist Rodney Needham, vertical classifications are not necessarily binary but may well be perceived as continuous. Ideally a moral order may be discontinuous, people are conceived as deviant or conventional, as outsiders or insiders, as crazy or normal. But in actual fact we tend to think that people are not likely to be all good or all bad. In the same way, even though the sociopolitical order tends to be perceived in terms of bipolar divisions between the rich and the poor or the oppressors and the oppressed, such dichotomies tend to be softened by the recognition that there are intermediate strata and graded positions. He argues, and here I find his argument not entirely satisfying, that binary categories, to use

the language of gestalt psychology, stand out as the "figures" against a "ground" of continuous coding.

Using some of my own work as a base, Schwartz also alludes to the circumstance that various strata and classes may prefer to think in continuous or discontinuous categories about the stratification system in terms of their own pursuit of sociopolitical goals and interests. Binary classifications, he argues, are selected from other faculties of the mind and exploited for a variety of human purposes. Humans seem to prefer to think in terms of binary opposites, but, as opposed to Lévi-Strauss, Schwartz does not see this thinking as biologically constrained. He believes they can think in a continuous mode when it suits their particular purposes.

I find Schwartz's argument persuasive. Yet I believe he has missed a crucial link in his reasoning. Children do seem to think in fused and global categories, so that what is above and vested with power is also seen as good and beautiful. But with growing maturity comes the power to differentiate and to distinguish the moral and the cognitive dimensions. Hence, while adults may still have a powerful tendency to think in terms of links between these dimensions, as Schwartz abundantly documents, they are no longer totally constrained to do so. They may at times resort to continuous and graded classifications, and they may also at times reverse the "natural" tendency to associate that which is above with the morally superior.

It is impossible to do full justice to the intricately reasoned and dense structure of this work within the space of a relatively short review. So let me only mention one other problem raised by Schwartz's neglect of the adult capacity to differentiate. He notes, without however giving the matter adequate attention, that the vertical, hierarchical classifications with which he deals here are especially congenial to conservative thinkers who use them to bolster a system of wont and use that is congenial to their interests. It pleases them to assert that those who have social power are also morally superior. Yet those who speak for the socially inferior have often thought in precisely opposed categories. To these spokesmen, even though the oppressed may at times adhere to a code of deference, they remain the repositories of virtue and will ultimately overcome the vices of the powerful and inherit the earth. Even though Schwartz alludes to such rival definitions, they do not receive enough attention because he tends to neglect the ability of adults to differentiate between moral and cognitive categories. In the same way, when it comes to cognitive rather than moral discriminations, innovative thinkers have often stressed the superior explanatory power of the lower rather than the higher regions of personality or social system. To Freud, the unconscious id rather than the conscious ego provides the dynamic element in personality; to Marx, the infrastructure rather than the superstructure reveals the dynamics of class conflict. To Noam Chomsky, deep rather than surface linguistic structures reveal the power of the mind.

But these are minor blemishes that pale into insignificance in the con-

text of the book as a whole. It is a towering achievement and its seminal ideas are bound to invigorate the sociology of knowledge for a long time to come.

Tradition. By Edward Shils. Chicago: University of Chicago Press, 1981. Pp. xiii+334. \$20.00.

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Ever since the Enlightenment of the 17th and 18th centuries, progressive intellectuals have looked upon traditions as obstacles to the realization of a just and rational social order. Marx was expressing the consensus of 19th-century progressives when he said that the tradition of all the dead generations weighs like a nightmare on the brain of the living. All this is not surprising, for during the struggle for the establishment of a modern civil society, traditions, or traditionalism, have become identified with ignorance, superstition, and aristocratic privilege supported by a reactionary Roman Catholic church. In *Tradition*, Edward Shils argues that the social sciences, which developed under the influence of the Enlightenment, have paid practically no attention to the nature and function of tradition and its impact on modern society. Even such a historically oriented thinker as Max Weber assumed that modern societies move toward the rational organization and control of all aspects of life and that the processes of rationalization and bureaucratization reduce the role of tradition to insignificance. Shils remarks that traditions have now fallen into such disrepute that it takes special intellectual courage to be reactionary.

This book presents a detailed and sometimes passionate plea for a serious, scientific, and political consideration of tradition. One problem with such an endeavor is that it is often very difficult to discern where science ends and ideology begins. On the science side, Shils's book makes a significant contribution by providing an analytic framework, richly illustrated with examples, for the study of tradition. He considers traditions as guiding patterns passed down from previous generations. These inherited patterns of judgments and evaluations are present in various degrees in all societies. Traditions represent a complex sequence of events and actions which become incorporated into such institutions as the family, the church, political parties, art, science, technology, and so forth. The endurance of past practices and objects allows the anticipation of experience and its legitimation, insuring the continuity of societies and institutions. Traditions have such a grip upon present experience that no human imagination can produce something wholly new, a *tabula rasa*.

Shils argues correctly that the rigid juxtaposition of traditional societies with modern ones by such writers as Tönnies, Simmel, Spengler, and

others has resulted in an overestimation of the force of tradition in "pre-modern" societies and an underestimation of traditions in modern ones. The difference between the traditional and the modern is never absolute but a matter of degree. What is unique about modern Western civilization is the growing antagonism between two different traditions. While substantive traditionality embraces the accomplishments of the past and traditional institutions as valid guides for the present, the tradition of reason and science accepts only facts that are the results of empirical observation and logicomathematical analysis. Furthermore, ever since the Enlightenment, liberal scientism has maintained that reason and knowledge will emancipate man from the shackles of tradition. In the 20th century, the tradition of reason has been utilized to legitimize an emancipatory ideology advocating equalitarianism, the relaxation of restraint of sexual activity, the diminution of parental authority, condoning attitudes toward criminality, and the rational management of society. Shils maintains that these programs have relatively little to do with reason or science except that they are also hostile to substantive traditionality.

Shils makes a strong case for the power of substantive tradition by showing the resistance of non-Western and even Eastern European societies to governmental efforts to rationalize or modernize institutions and conduct, although one may argue that ever since the 16th century Western governments have encountered very similar resistance to their attempts at economic and political rationalization. Not only in the East, but also in the West, reality has always fallen short of the "ideal" of rationalization. Following Edmund Burke, Shils argues that substantive traditions represent the accumulated wisdom of the ages, arrived at through the trial and error of many generations. This may or may not be the case. But Shils also argues that because certain beliefs, practices, and institutions existed, their existence indicates that they served those who lived in accordance with them. This is certainly not always the case. Countless examples may be given to the contrary.

Unlike substantive traditions which evolve more or less spontaneously, scientific traditions are characterized by deliberate change or progress. When extended to society, progressivism considers the latest forms of society more desirable than earlier ones and advocates the utilization of empirical knowledge and abstract, rational principles in the service of the attainment of ends that are also subject to rational evaluation. Shils is certainly not an irrationalist. He considers any tradition that encourages the exercise of the faculty of reason a highly desirable one. Nor is he a reactionary, for he has a positive view of science, and of the "tradition of emancipation from traditions" that transformed serfs and slaves into citizens. However, he argues that, under the sponsorship of "collectivistic liberals," "social democrats," "radicals," and the "lumpen-intelligentsia," emancipation has gone too far. It has damaged the traditions of religious and family life, it has undermined authority and has encouraged extreme individualism. In order to reverse this trend, Shils advocates a more "mod-

erate individualism" that is sustained by substantive traditionality and also has a more limited rationalization, one that no longer considers traditions as obstacles to progress but as possible guidelines for future action.

Shils dedicates his book to T. S. Eliot and Max Weber, and the work may be seen as an attempted synthesis of Eliot's advocacy of traditionalism and elitism with Weber's resignation to the process of rationalization. As such, this work is a failure. Eliot was an authentic reactionary who developed his view of society under the influence of Charles Maurras and the *Action Française*. Weber, like Marx and Durkheim, considered many aspects of modern capitalist civilization undesirable, but he was too much of a social scientist to become a reactionary. Since he did not believe that socialism could resolve the social and human problems of modern civilization, he developed a sense of resignation and advocated an ethics of responsibility. What Marx, Durkheim, and Weber had in common was the conviction that the problems of modern civilization are structural problems and any solution for them would require structural transformations. Thus, the breakdown of traditional values and social aggregates, the changing role of the state in civil society, class conflict, narcissistic individualism, anomie, disenchantment, the commercialization of culture, and so forth, are seen, not as the products of certain "ideals" advocated and pursued by a select group of intellectuals, academics, or politicians, but as things endemic to the nature of modern capitalist civilization. In this sense, Shils's advocacy of a return to a "moderate individualism" rooted in substantive traditionality is no less quixotic than Eliot's notion of the restoration of a "Christian elite" to its supposedly rightful civilizing mission. But, unlike Eliot, who as a royalist, Social Creditor, and neo-Thomist was critical of the structure of modern capitalism, Shils seems to embrace it. What he rejects is its culture. However, an advanced capitalist society rooted in substantive traditionality—that is to say, in essentially precapitalistic values—is not very likely to emerge in the foreseeable future.

Puritan Boston and Quaker Philadelphia. By E. Digby Baltzell. New York: Free Press, 1980. Pp. xii+585. \$19.95.

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This is a tale of two cities as seen through the special lens of the propensities of their upper classes for leadership. The author, a native of one city (Philadelphia) and an admirer of the other (Boston), sets forth a bold, if controversial, hypothesis which he pursues with passion and imagination.

Puritan Boston and Quaker Philadelphia is a work of scholarship and

social history with a boundless array of fascinating details about individual personages and social events. I found the social history much more compelling than the social theory for reasons I shall explain further on, but all told, in this important work E. Digby Baltzell pursues arduously the complex task he sets for himself.

This task is, in the author's words, "to compare and contrast two privileged classes in order to show why Boston Brahmins produced a long tradition of class authority whereas proper Philadelphians did not" (p. 5). The principal reason lies in the very different religious ethics of the founding fathers of each city, a conclusion Baltzell had reached intuitively before he began the book but which he proceeds to document in abundant detail throughout this lengthy volume. These ethics are seen to have had a decisive influence on each city's style of public leadership and achievement for many generations. Puritan Boston, in contrast with Quaker Philadelphia, has had an unmistakable "leadership superiority," which reflects the authoritarian and hierarchical tenets of its creed.

At first glance, Baltzell seems to propose something akin to a sociological experiment, with his comparison of two transplanted ethics in two major cities at the same historic epoch and stage of the society's development, and of two upper-class leadership styles. However, whether this comparison permits the testing of hypotheses about the "influence of ideas and values on social structure and character formation" depends on the extent to which the two situations are seen as truly identical, with the key variable controlled. Unless this is true, the author's objective must be more modest.

If one examines the stated thesis of this book with care, it is evident that it is not one thesis but three. The first is that an upper class steeped in authoritarian and hierarchical values is likely to produce strong leadership through many generations, whereas one imbued with democratic and egalitarian values is not. This first thesis is then used to account for the historic leadership styles of Boston and Philadelphia. A second and quite different thesis asserts that materialism is inimical to authoritative leadership, and since the "virus of virtuous materialism" afflicted Philadelphia for three centuries, the "best men" of Philadelphia "have seldom sought public office or positions of societal authority and leadership *outside business*" (p. 9; my italics). Here, then, the distinction between materialist and nonmaterialist values becomes decisive for an understanding of class leadership, although here such leadership excludes business. A third thesis pits individual desire against class duty and pride against snobism as explanations of why Boston's dutiful upper class sought public and intellectual achievement while snobbish Philadelphia's did not. All three theses are interesting, yet they remain only suggestions because the evidence needed to substantiate them is not presented.

One of the high points of the book is a series of marvelous vignettes contrasting persons who are representative of each city's moral and cultural milieu, as, for example, Oliver Wendell Holmes of Boston and John

Grover Johnson of Philadelphia. They were born in the same year and succeeded in the same profession but had very different ambitions. Where Holmes sought fame, Johnson sought wealth. Though each was a religious skeptic, according to Baltzell their values bore the imprint of the religious climate into which each was born. However, there may be other factors than ancestral creed that determine an individual's goals. To decide whether conformity to social values or personal propensity better accounts for the differences between Holmes and Johnson would require biographical information about family, upbringing, and character not discussed by Baltzell. The same is true of his other comparisons—John Winthrop and William Penn, Thomas Hutchinson and Benjamin Franklin, Henry Cabot Lodge and Boies Penrose—all are fascinating and stimulate numerous questions but do not clinch the argument.

In all of these comparisons, as in the book as a whole, Philadelphia takes something of a beating. So do proper Philadelphians, who are seen as snobbish and materialistic while their Boston counterparts emerge as proud and principled. If these conclusions seem unpersuasive, it is because a host of unexamined assumptions about authority and leadership underlie the argument. Obviously, Baltzell considers only certain arenas of leadership as appropriate and acceptable. So when, early in the book, he poses the tantalizing question of why 18th-century Philadelphia, despite all its wealth and splendor, did not produce "great leaders," one would appreciate a specification of his leadership criteria. That is also true for other forms of prominence, referred to but not defined, and often used interchangeably, such as authority, excellence, superiority, fame, and success.

More troubling, to my mind, is the omission of the concept of power from a discussion of class rule, suggesting a rather one-sided perspective on that matter. This perspective attributes morality and honor to upper-class authority (as in Boston) but sees the democratic elitism of Philadelphia as conducive to anarchy and chaos. These ideas, grounded in 19th-century perspectives on elites, are never fully developed but remain as ideological undertones. Every so often I was tempted to reverse the comparison and, instead of taking Boston as a standard that Philadelphia is unable to match, make Philadelphia the standard for Boston.

Philadelphia gets short-changed because the values and life-styles guiding its upper class go against the connection that Baltzell stipulates must exist between social order and class authority. By that criterion, Philadelphia is bereft of leadership. Yet how does this fit with a contrary claim that each city produced its own particular brand of leadership: Boston, "men who excelled in arts and letters," Philadelphia, men of innovation and change?

Indeed, the contrast between the two cities tends to be overdrawn. If it is true that "in both cities the majority of upper class members rest on their privileges and do not become leaders in government and society" (p. 33), then in what sense can the ambitions and achievements of a

minority be traced to a religious ethic guiding the majority of the class?

Another methodological issue requiring clarification concerns the criteria for sample selection used in the quantitative analysis. Much of the discussion rests on entries from the *Dictionary of American Biography*, on subsamples of 50 families in each city with genealogies spanning two centuries, and of listings in *Who's Who* and the *Social Register*. Students of elites are all too familiar with the intractable problem of cutting points and boundaries for the demarcation of elites. Since there is no foolproof method of selection, the particular method used here requires more explicit discussion than it receives. In addition, in a book that strives to demonstrate a causal connection between religious values and class authority, it is regrettable to find no actual data on such a connection, which therefore remains highly inferential.

Thus, while this book is fascinating and engrossing, its strength lies in its descriptive narrative of men and events rather than in the demonstration of its key proposition. Moreover, in idealizing Boston's upper-class rule—"authority is a product and producer of love and trust"—Baltzell neglects the individual and social costs of such a rule and plays down its familiar faults—arrogance, intolerance, lack of the common touch, and repressive treatment of the young. One wonders how many scions of old Boston failed to thrive in such a rigid authoritarian atmosphere, and at what price others succeeded.

Thus, while Baltzell has convinced me that Boston's upper class stressed intellectual values and public achievements, I am not convinced that this emphasis makes it superior to Philadelphia. Not only was Philadelphia ahead of Boston in the colonial era, but it also continued to produce leaders of science and industry throughout the 19th century even after losing out to New York following the failure of the Second Bank. If the business of the United States was business, it was Philadelphia that provided leadership in that area.

The special pleading for the virtues of an aristocratic social order, interspersed throughout the book, also extends to special warnings about "fanatic forces of egalitarian individualism" that Baltzell holds responsible for a host of contemporary social ills. By interweaving these biases into his analysis, he often loses the value neutrality associated with one of his intellectual models, Max Weber. His preference for traditional class authority over accessible elites condemns Philadelphia's upper class as anarchic and irresponsible while Boston's stands exalted. In that sense the book at times sounds more like an apologia for aristocratic authority than the objective assessment of authority it tries to be. In any case, authority structures do not stand alone and cannot be superimposed on social systems at will. Even the Puritan upper class was pushed aside by new social forces and new codes and styles of leadership, and while it may have had more family cohesion and continuity than Philadelphia, Boston, too, had to give way.

I want to conclude this review on a note of appreciation for this work.

Studies of elites and upper classes are few and far between, and Baltzell has made singular contributions to this vital if neglected area. If I am critical of certain features of his exposition, I must also underscore his considerable achievement in this detailed comparative study of two significant elites and the ideas, ideals, and institutions that shaped them.

Books: The Culture and Commerce of Publishing. By Lewis A. Coser, Charles Kadushin, and Walter W. Powell. New York: Basic Books, 1982. Pp. xiii+411. \$19.00.

Douglas Mitchell
University of Chicago Press

Because this sociological analysis of the interplay between spiritual and material aspects of contemporary American life focuses on one specific "institution"—the world of publishing—*Books: The Culture and Commerce of Publishing* will have at least potential appeal to any serious author or reader of books; it will also be read avidly by many of the insiders—people who edit, promote, sell, and review books—whose world has been closely scrutinized here by Lewis Coser, Charles Kadushin, and Walter Powell. The framework the authors bring to their study is that of organization theory, currently the dominant social science perspective for studies of the "production" of culture; hence there will be additional readers in those subdisciplines. Coser, Kadushin, and Powell tell the story of publishing in America primarily from the point of view of publishers themselves, but they argue that their organization theory/participant-observer perspective deepens our understanding, whether from the outside or the inside, of a crucial industry in its role as cultural gatekeeper. For this reason they can sympathize with publishers' laments about the gloomy status and fate of book publishing (fears about the leveling of taste and quality in the face of gimmick books, blockbusters, pandering to a mass audience, and so forth), while at the same time keeping a clearer eye on the past and present realities of an industry that has heard these laments many times before and whose problems are not unique in our society. This detachment, involving the spiritual/material theme, or the "culture and commerce" of the subtitle, is summed up in an assertion concerning modern industrial societies, which are "plagued with a basic contradiction: while the logic of mass production homogenizes tastes, the relative affluence of the society and the complex division of labor gives rise to specialization and differentiated tastes. There are numerous communities in American society with their own tastes and cultural preferences, many of them quite sophisticated. Publishers are not alone in having difficulty trying to reach these communities in an economically viable manner" (pp. 373-74).

While Coser et al. are mindful that publishing is one among several



cultural industries, the burden of their analysis hinges on their perception of why publishing is nevertheless distinctive as an industry. They ascribe four characteristics to it that underpin the whole account. First, the audience or market for books has been, is now, and will always be a mostly unknown quantity (i.e., unknown for this or that particular book, with the possible exception of introductory textbooks, where market research can sometimes appear to supply known quantities). Book publishing "is a highly uncertain business in which the supply of commercial manuscripts cannot be guaranteed and consumer demand is fickle and unpredictable" (p. 127). Second, the industry of publishing is decentralized and therefore involves plural sectors, which the authors lay out in a recurring (and sometimes annoyingly redundant) threefold schema of trade-scholarly-college textbook publishing. This schema, willy-nilly, systematizes much of what they have to say (although they do not argue that it is inclusive, about which more below). Third, publishing provides a mix of both modern mass-production methods and of craftlike procedures—which is to say, corporate rationalization and bureaucratization still coincide with/clash with individual intuition and creativity. Last, publishing, like some other industries, but perhaps more so, represents an always tense balance between the restraints of money-making and the obligations of guarding or advancing the state of a society's culture.

The lengthy account which evolves from this four-part organizing assumption is laden with amusing anecdotes, sometimes insightful analysis, and occasionally leaden homogenizations of statistical samples. The authors make a strong start—theirs is a sociology blessed with a sense of history. They show convincingly that, for at least two hundred years, publishing in this country and in Europe has always experienced a conflict between the pressure to make profits and the integrity of publishing quality books. Most American trade publishing, for example, has been aimed at a mass market: to exemplify, they chart at least three paperback "revolutions" prior to our day (1840s, 1860s, late 1940s), and trace the history of publishing mergers, especially common in the 1880s (and at some points amounting to virtual textbook monopolies). Although their historiography is a bit naive—positing a dyadic model of straight-line progression versus cyclical evolutions—they make a case for the appearance of swings back and forth between competition and oligopoly. This history permits them to remain relatively sanguine about the present and future of publishing—for instance, they do not foresee the complete demise of the hardcover book, if only because reviewers do not generally take original paperbacks seriously—while at the same time they can caution us about the impact of the blockbuster ("all or nothing publishing"), with its attendant erosion of contact between publishers and authors (literary agents are the middlepersons) and shift in source of profits from sales to bookstore customers to sale of movie rights, TV rights, T-shirts, bookclubs, and so on.

Coser et al.'s data base (and methodology), aside from frequent citations of sources like *Publisher's Weekly*, was drawn from over a hundred

intensive interviews with book editors, systematic observation of 10 publishing houses, and several hundred questionnaires (to editors and other publishing personnel as well as to authors working with publishers). (As a measure of their skill as participant observers, they note modestly that half of their field observers were offered jobs as publishers' editors and that another third of the team was approached indirectly about taking positions in publishing.) Despite blurb copy on the book jacket, however, they do not claim to be covering the entire industry. Their focus is on nonfiction, and within publishing houses their observations centered mostly on the editorial process. There is little or nothing in this account which addresses problems of elementary/high school, juvenile, or reference-work publishing, nor did they attempt to pursue the internal issues of book manufacturing and production, copyright and contract work, book clubs, computer technology, or international sales. Some will find these omissions damning; I think that the results of what they *have* studied are sufficiently systematic in their implications to be productive of much more work on publishers as organizations and, later, on publishers vis-à-vis larger questions of culture and the creation of values. What follows now is a brief sketch of a few of their conclusions.

The authors argue at some length on behalf of the value of their three-way division of publishing into trade, scholarly, and textbook concerns (although "scholarly" is not always used precisely when they deal with differences and similarities among university presses, commercial professional presses, and the scholarly aspects of trade publishing). One might object that they overdo the argument, especially in the "Setting Standards" section (pp. 61-68), where they attempt an oblique factor analysis of how editors rate other houses in a variety of clusters. The need for this section, not to say its clarity and utility, escaped me, and, given the choice, I would have eliminated it. In practice, however, the tripartite schema seems to work well for them. Because the product derived from each kind of publishing house is different and has a different sort of audience, it follows that editorial practices will vary in each case—for example, marketing can be instrumental in the selection of textbook projects, whereas in trade publishing the increasing importance of literary agents (as stand-ins for authors) and subsidiary rights managers threatens to eclipse the editor's selective function; only in scholarly publishing is the editor still king. The reasons for these differences follow easily: textbooks are increasingly targeted to introductory courses, instructors (who can be polled by market researchers) are their real audience, hence the market is less fickle, but the initial investment in producing a textbook is so great that it requires a strong dose of marketing input; trade books are more and more prone to "tie-ins" (involving subsidiary rights), with a resultant increase in financial stakes which requires signing up authors who have established track records. All this takes place in an atmosphere that Coser et al. characterize as a dissolving network of the literary contacts (primarily in New York City) that once provided close circles of authors,

editors, and book reviewers. Meanwhile, the financial risk taken on most scholarly books is comparatively very low and scholarly book editors can still (though not as readily as in the past) rely on "invisible colleges" of creative scholars who are on the leading edges of their disciplines and who can consult with editors on promising new work (and at the same time spread word to writer-colleagues about the reputations of sympathetic editors).

Coser et al. make good use of the three-way schema to show how it affects author-editor relations; influences initial press runs, authors' advances, and shelf life of new books; launches career patterns for people in publishing (particularly for acquisitions editors); shapes the day-to-day routines of working editors in the various kinds of houses, including the imposition of quotas for the number of books management expects editors to sign in a season or year; establishes criteria for the decision to publish (including the impact of academic status of authors on that decision); molds the interdepartmental organization of publishing houses; dictates the amounts spent and the ways they are spent in marketing books; and directs the differing influences of sales and publicity in trying to reach audiences for the different types of books put out by the three kinds of houses. There is also an interesting chapter (separately authored, apparently, by Michele Caplette) on the career patterns of women in publishing, which does not make much use of the triadic schema, focusing more on college textbook publishing, and another chapter on the author's view of publishing that stresses the asymmetrical power relation between author and publisher and brings the schema back into play, if only indirectly, in assessing why an author does or does not remain loyal to a publisher.

In sum, this is a thorough and insightful study of a world which has had, until now, even among its own inhabitants, too much the aura of mystique and of self-congratulatory or self-deprecatory preachments without benefit of hard analysis. As I mentioned above, there will be brickbats thrown—sometimes fairly, more often unfairly—at Coser, Kadushin, and Powell. As an acquisitions editor for a scholarly press, I, for one, would quarrel with their conclusions about how editors perceive the ways they spend their time. From my perspective, university press editors spend enormous amounts of time in recruiting outside readers for new manuscripts, and they also put in many an hour on the telephone and in correspondence, as well as in dickering with both authors and their own contracts/subsidiary rights departments on contract terms and contract preparation, but my own case would of course be merely one small instance compared with their larger sample of scholarly publishers, not to mention the other kinds of houses. (The authors claim that editors mostly shun the telephone and typewriter—here I expect they must be talking about trade editors—and that they do spend lots of time with authors and reading manuscripts, which, in my experience, is not so true of scholarly press editors. On the other hand, my prior experience in textbook editing sug-

gests that their perception of textbook editors as addicts of meetings is fairly accurate, given the fact that textbook work involves careful coordination of schedules for finance, editing, production, and marketing.) One other issue will probably be raised in some reviews of this book: Whom do *they* perceive to be their audience? The "they" here is probably the editors at Basic Books who selected and published this book. To their real credit, Basic has put a good list price on an expensively designed and produced book (the per page cost alone is impressive). Basic has also sought to integrate the styles of three different authorial voices—a laudable goal, one might suppose. But the results of this effort are likely to draw some fire; the conglomerated style is gray—reminding me of a writer friend's description of "scholarly" prose as being written in the fourth-person/inert voice—and there is a cloying tendency to put familiar terms or phrases in quotation marks and to insert, for highbrow flavor, literary quotes from such people as Malinowski or Ortéga y Gasset (identified respectively as "anthropologist" and "Spanish philosopher"—identifications which any serious author, reader, or publisher of books would take for granted). Such condescension may border on the fatal in the short run. In the long run, this book will stand as a contribution to sociology and to the critical cultural study of American life.

History of the Idea of Progress. By Robert Nisbet. New York: Basic Books, 1980. Pp. xi+370. \$16.95.

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Robert Nisbet has written an intellectual history of the old-fashioned, author sitting in an armchair viewing from Olympus variety. *History of the Idea of Progress* reflects the occasional glories and more usual failings of the genre: random moments of depth and particular insight, and encyclopedic breadth, on the positive side; argument by selective quotation, disregard of historical and intellectual context, and arbitrary and inconsistent classification, on the negative. Nisbet has a meta-thesis: the idea of progress in the West has been and is dependent upon Judeo-Christian belief (although his own misguided celebration of classical belief in progress ought to have precluded that conclusion). Nisbet seeks to convince us that belief in gradual human progress is not a relatively new idea in the West, born of a rejection of classical and Judeo-Christian roots. This thesis would be welcome as a corrective of simplistic and unnuanced readings of the complex of diverse classical and Christian traditions. However, Nisbet creates a history that suffers precisely from oversimplification, lack of nuance, and a partisan spirit.

Instead of confronting the ambiguities of the theme of progress amid the diversities of the classical world, Nisbet marshals sentences and para-

graphs from selected texts without regard for the broader concerns of an author's work, for time and place, or for contrary evidence. To discuss Hesiod and ignore Pandora is to beg the issue entirely. By Nisbet's criterion, a belief in progress should reflect an affirmation of both gradual, past historical advance and future ascent. In categorizing the ancients, however, he seizes on any theoretical optimism for the future, even if it is linked with belief in prior historical degradation, and on any assertion of prior positive development, even if it is linked with the profoundest pessimism about the future. Broad currents of Greek thought—the Skeptics, the Cynics, the historically pessimistic Peripatetics of the later period—are excised from the record. More disturbing, he conveniently forgets, even among many of the thinkers whom he cites as believers in progress, any tendencies toward resignation, fatalism, and contempt for life and the world.

A historian who set out to construct a history of historical pessimism and utilized the same methodology on the same sets of authors would have had an easier task, since pessimism prevails in the chapters devoted to early and medieval Christian thought. It is not simply, for example, that Nisbet can emphasize Saint Augustine's belief in stages of human development and ignore the implications of the final stage being *senectus*, nor that he can focus on the most minor currents and asides of Tertullian's thought while disregarding the center of it. Nisbet has an almost willful historical blindness about the entire context of the Christian doctrine of the Fall, of the catastrophic secular meanings of eschatology and of a moral philosophy of earthly resignation. To him, the Scholastic ontology which equated mutability with corruption is irrelevant. His arbitrary construction of scholars' attitudes toward the place of knowledge in human affairs ignores an intellectual world centered upon the *disputatio*, in which citation of ancient authority took absolute priority in argument and conviction. That the Scholastics saw themselves as dwarfs on the shoulders of giants is for Nisbet yet further proof of their belief in progress: after all, a dwarf on a giant's shoulder sees farther. (Now if only they had believed in the strength of the shoulders of the dwarfs!) It is enough, for Nisbet, that they believe that *fide* and *ecclesia* will prosper under the particular and general providence of God. In all other concerns, he presents the atypical as representative and, unwittingly, makes the Pelagian heresy the orthodox creed.

If the medieval Christian world was the bearer of belief in human progress, then, for Nisbet, the Renaissance rejectors of that world rejected that belief as well. By counting Francis Bacon among such Renaissance minds, Nisbet transforms this central figure in the creation of the modern belief in progress into a disbeliever in gradual human amelioration. Defining progress as entailing a gradual ascent to any present, Nisbet disallows for Bacon and his ilk (though emphatically *not* for the Fifth Monarchy men—compare pp. 102–3 with p. 139) the compatibility of belief in progress and a sharp rejection of a preceding age. Thus, Nisbet fails to see how the rejection of the presumptive authority of the past

constituted the most distinguishing characteristic of the revolution in European thought that shaped a new countervision of human possibilities: naturalistic, secular, and inextricably bound up with notions of human mastery and a rewriting of the sorry scheme of things. Instead of unraveling the new dynamic in thought about past and future made possible by this revolution in both theological and humanistic conception, Nisbet remains bound by his *idée fixe* of continuity of a belief either embraced or rejected.

For a generation of intellectual historians influenced by Lucien Febvre's insights into the discontinuities of climates of meaning, by Quentin Skinner's notions of context, and by J. G. A. Pocock's strictures on the "multivalence" of ideas, Nisbet's work will be a major disappointment. In writing of thinkers whom he wishes to rescue from misappropriation—Mill and Spencer, for example—Nisbet pays careful attention to the broader context, the author's specific linkage of ideas, and the nuances of a text; in rejecting certain readings of Hegel, he understands full well that "freedom" (why not *progress*?) is a notion of often mutually exclusive meanings and conceptual premises. Such exceptions to intellectual laziness, however, only serve to make more apparent all of the missed possibilities of this ambitious work.

A Search for Structure: Selected Essays on Science, Art and History. By Cyril Stanley Smith. Cambridge, Mass.: MIT Press, 1981. Pp. x+410. \$30.00.

From Art to Science: Seventy-Two Objects Illustrating the Nature of Discovery. By Cyril Stanley Smith. Cambridge, Mass.: MIT Press, 1979. Pp. 118. \$25.00.

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Cyril Stanley Smith began his long and distinguished career as a metallurgist and became a materials scientist when that field broadened. He simultaneously cultivated interests in the history of technology, which eventually became his major concern, and in the history of art, to which that led him. *A Search for Structure* contains 14 essays, many of them quite long and all extensively illustrated, containing his ideas about art, science, technology, and structure. *From Art to Science* is the catalog of an exhibit of archaeological, artistic, and scientific objects which Smith assembled to illustrate those theories. Smith's thinking is not really sociological, so his essays are less directly usable by sociologists than evocative, filled with ideas and speculations that suggest related notions in matters that concern sociologists, particularly sociologists of art and science. Here is a précis of the themes that run, in several versions, through these essays.

Smith is concerned with the scale of what scholars study. He thinks science studies small atomic structures that are neat and orderly (e.g., the lattice structure of crystals) or very large structures with a few units (the solar system). But it has not paid much attention to objects at the human scale, *materials* rather than *matter*. The people who work with materials are artists and technologists. Technology, which deals with the objects people use and the qualities of the materials they are made of, relies less on scientific knowledge than on sensual experience and intuitive, practical, concrete knowledge. Smith reminds us that, at least until recently, most technological advances were made long before scientists understood why these things worked. Smith's background leads him to such obvious examples as the invention of various alloys. Artists, too, are intimately involved with materials—metal, marble, wood, paper, canvas, pigments, photosensitive emulsions—and in making objects, and so technology is closely related to art.

The elements described in and explained by scientific theories are simplified, abstract, and ahistorical. On the other hand, the objects technologists and artists make are complex. Materials are not the same as matter. Matter is what science is about. Smith uses a recurring image to illustrate what he means by matter: the regular lattice structure of a perfect crystal. Materials may be made up of such crystals, but the crystals do not fit together neatly. Materials have all sorts of irregularities, which occur when perfect crystals do not line up perfectly, each leaving a zone of disorder at the edge where the two arrays meet but do not mesh. Such zones of disorder are common to all materials and, in fact, it is the presence of such zones, perhaps in regular patterns of their own at a more microscopic level, that gives materials their characteristic traits—metals their hardness or ductility, for instance. These irregularities come about through things that happen to the material as it is heated, cooled, hammered, bent, and so on. All those events leave their marks in the microstructure as well as on the surface, and the object is thus a record of its history. This sort of relationship between elements at different scales exists at every level of size, but it is most important in the objects we use. In other words, objects have an internal order which is disrupted by their meetings with the order external to them, so that relations with the outside affect the way the inside looks and works. Conversely, the character of their inside constrains objects' relations with the outside. Smith sees this order at work in art, in biology, in society—it is an intriguing metaphor whose implications for sociologists' thought are not immediately clear to me. The analogues are not easy to see but are worth thinking about all the same.

It follows from all this that historians of art and science focus too much on theories and ideas (iconography, scientific writing) and not enough on things, on the objects people make. In his own field—and this is what led him to take history so seriously—Smith found that you could not write a satisfactory history of the development of metals and metalworking using written sources, because sufficient sources did not exist (craftsmen

being more likely to make things than to write about how to make them). Instead, he went to objects, whose structures contained the history of "their making." Subjected to microscopic and other analysis, they tell you what people must have done to make them as they are.

Smith's investigations of early metal objects showed him that many of the objects he found interesting for his work were not "useful"—that is, in the early societies which produced them they were not used in agriculture or war. Instead they were made for decoration or adornment and were found, not in the main galleries of art museums, but in the closets reserved for the minor and decorative arts. This leads him to one of his wittier speculations: that early people must have developed metals (and other materials) for nonserious purposes and then adapted them to more important uses later. After all, you would not want to risk making anything important from a new substance you could not depend on.

This leads, finally, to Smith on art. He wants art historians (we can substitute sociologists of art with no harm to his point) to pay more attention to the materials artists have used and the way those materials have affected what they make, even if it means giving less importance to artists' theories about what they are doing and to the subject matter and symbolism with which they work. He has a nice way of thinking about artists interacting with their materials. Remembering that materials, after all, have physical properties, he says that an art work contains the signs of the artist coming to terms with the material so that what it wants to do is what the artist wants it to do. Smith's suggestions about art have the virtue of giving real importance to what artists think is important. This is not to say that they necessarily know best, only that we ought not to ignore what is obvious to everyone involved in these activities.

This summary and appreciation does not do justice to what reading the whole work and puzzling over the objects in the catalog and in the illustrations to the essays will give you. In the nature of the medium—essays and catalog—the argument is not tight and rigorous. But it should not be. It should be open-ended and ruminative, as it is, because it is the record of someone thinking over a body of extensive and profound experience, and that can best be appreciated by ruminating along with him.

Statistics in Britain, 1865–1930: The Social Construction of Scientific Knowledge. By Donald A. MacKenzie. Edinburgh: Edinburgh University Press; New York: Columbia University Press, 1981. Pp. viii+306. \$25.00.

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Readers of standard textbooks in statistics are introduced to Pearson's product-moment correlation, "Student's" t , and Yule's Q . Each bears the

name of its inventor, but each is presented as an impersonal and timeless truth. In Donald MacKenzie's book, these statistics become historical artifacts in need of sociological reconstruction, and we learn much about the men behind the eponyms. Although we are told, for example, that R. A. Fisher showed his impatience with boring colleagues by removing his false teeth for cleaning, this is no mere scandal sheet for the giants of statistics. *Statistics in Britain* is a careful and largely successful effort to describe how social interests affected the conceptual development of statistical theory in Britain from 1865 to 1930.

MacKenzie's study is best located in the historical sociology of scientific knowledge. He manages to avoid two traps. His is not the wild speculation characteristic of early sociology of knowledge, where the "structure" of knowledge is discovered to be isomorphic to the "structure" of society (perhaps only because these structures are so ambiguously defined that the conclusion becomes a sure thing). Marx, Durkheim and Mauss, Scheler and Mannheim have at last become forerunners to a more sophisticated sociology of knowledge. Neither does the study get so caught up in details that it becomes just another biography or intellectual history. Eight years of scholarship enable MacKenzie to tell a story of Francis Galton, Karl Pearson, and Fisher that has explicit theoretical as well as descriptive goals.

The theoretical task is to identify connections between beliefs and social positions by specifying how "interests or experiences constrain the set of beliefs 'appropriate' to occupants of these positions. 'Appropriate' beliefs will be ones justifying a group's privileges, advocating an advance in its situation . . ." (p. 5). Scientists, too, have social interests, and these constrain their beliefs about nature as much as social interests constrain the exhortations of ideologues. The social and intellectual history of science is thus to be read as the accumulation of day-to-day decisions constrained by identifiable social interests of scientists. This orientation has been identified as the "interest model," part of the "strong program" in the sociology of knowledge being developed by Barry Barnes, David Bloor, Steve Shapin, and David Edge at the Science Studies Unit in Edinburgh, where MacKenzie is lecturer of sociology.

MacKenzie is quick to tell us what his theory does not claim. Relations among social positions, interests, and beliefs are not deterministic: MacKenzie rejects the simple logic that equates occupancy of a social position with a necessary set of beliefs. Scientists freely invent theories, but choices are made within social contexts that define only some options as means for furthering their wealth, prestige, power, or security. Second, validity of MacKenzie's theory does not depend on empirical proof that "at least half" of those who occupy a position share a belief. The argument is less statistical than structural in the manner of Georg Lukács and Lucien Goldmann, seeking explanations for *why* an individual occupying a set of positions would adopt a set of beliefs. Finally, social interests do not necessarily

translate into motivations for belief or action. A variety of intentions could lead scientists to invent a theory (including, say "enhancement of explanatory power or technical precision"), and these may or may not coincide with scientific, political, economic, or ethical consequences of these scientific beliefs.

The theory sounds empty in the abstract, but it is quickly filled by rich empirical materials MacKenzie has gathered from correspondences, the contemporary scientific record, and an array of biographies and intellectual histories. His research site is a Golden Age in the history of statistical theory.¹ Many statistics we now use to crunch data were developed in Victorian and Edwardian England: Galton developed regression and correlation to permit bivariate analysis; Pearson developed the standard formula for the correlation coefficient and the χ^2 test for goodness of fit; Fisher developed the theory of statistical inference. MacKenzie gives a chapter to each of these central figures, and two more chapters describe a pair of critical debates. One debate set Pearson and fellow biometricians against Mendelian geneticists best represented by William Bateson. In the other, Pearson and his former student George Udny Yule debated how to measure statistical association with nominal variables.

How did social interests affect the invention of these statistics and the debates they inspired? MacKenzie's research site is hardly an easy case for this sort of analysis. Pearson's correlation coefficient, for example, has become part of the canon of modern statistics, and so MacKenzie must reveal how social interests are as inextricable from "truth" as from "error." Second, MacKenzie wants to go beyond sociological explanation of the growth of statistics as measured by number of publications or participants, and he tries to indicate how social interests affected the *content* of statistical knowledge. Finally, the social interests affecting the development of statistical theory are not only those from within the institution of science (for example, the greater prestige one enjoys by being the first to solve a hotly pursued problem). MacKenzie faces the challenging task of locating these scientists in a wide variety of extrascientific social roles. Does he succeed?

A key to understanding the scientific work of Galton, Pearson, and Fisher is their common advocacy of eugenics. Each favored policies for selective breeding to produce an English "race" better equipped to achieve imperialistic aims. Eugenics appealed to these statisticians because its success would advance their power, prestige, and security as members of a "rising professional middle class." Eugenics would require a shift in the locus of power: society would not be in the hands of an aristocracy with land and title, a bourgeoisie with capital, or a working class with physical labor power. Rather, society would be controlled by experts, those (like Galton, Pearson, and Fisher) with a "naturally" superior intelligence and capacity for learning. MacKenzie is not the first to note the commitment to eugenics by many members of Britain's intellectual elite, but he ex-

tends our understanding of how these political views affected the content of scientific knowledge. MacKenzie describes how the various statistics were developed as solutions to problems posed by the eugenics program—for example, the problem of how to measure the intergenerational transmission of intelligence. The evidence is convincing.

But the statisticians were not only members of a rising professional middle class, and the other social positions they occupied carried interests which further constrained their statistical theorizing. For example, MacKenzie traces the debate between biometricians and Mendelians, not to different “class” interests, but to different “occupational” interests grounded in different scientific skills. Bateson thought that biometricians lacked his professional biological expertise and Pearson thought that Mendelians lacked his mathematical rigor. Apparently, scientists will do almost anything to avoid the appearance of obsolescence. Thus, these statisticians were constrained by multiple and at times cross-cutting social interests, and this makes it difficult to generalize from MacKenzie’s case study. His explanatory use of “interests” is ad hoc: the strategy is to start with a contribution to scientific knowledge and then search the biography of the scientist until a social status is found that carries interests that are advanced by that contribution. Yule’s split from Pearson may have been due to Yule’s rejection of eugenics; Bateson’s statistics reveal a kind of romantic conservatism which resulted, we are told, from his near half-century at Cambridge. If MacKenzie succeeds in showing us that social interests constrain the construction of scientific knowledge, he provides little foundation for guessing *which* social interests—those attached to class, occupation, or university affiliation—will in general take precedence.

Statistics in Britain might prove unsettling for quantitative sociologists who happen to pick up the book. If MacKenzie is correct, social interests constrain our acceptance and use of certain statistics, just as social interests constrained their inventors. Few sociologists these days favor eugenics; what social interests are behind *our* support for the χ^2 ?

The Dynamics of Social Movements: Resource Mobilization, Social Control, and Tactics. Edited by Mayer N. Zald and John D. McCarthy. Cambridge, Mass.: Winthrop Publishers, 1979. Pp. viii+274. \$14.95.

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In the early 1970s Mayer Zald and John McCarthy proposed the “resource mobilization” approach to the study of social movements. Among the significant features of this orientation is a clear distinction between a social

movement as a whole (civil rights, for example) and the component organizations (CORE, SNCC, SCLC, NAACP) that play a prominent part in it. This focuses the attention of investigators on some problems that have been neglected in many previous studies, such as the manner in which each association mobilizes its personnel and other resources to carry on its struggle as well as on the succession of internal problems and tactical dilemmas that arise. This approach also underscores the relation of each group to the mass media, to authorities, and to the wider audience that may become involved. Most important, it makes possible an analysis of the manner in which the various organizations cope with one another. Although it is commonly assumed that groups striving toward similar goals will collaborate, this has not usually been the case. Indeed, many social movements that might otherwise have succeeded have been destroyed by in-fighting among rival organizations. Zald and McCarthy's approach thus facilitates viewing social movements within the context of the political processes taking place in the communities in which they operate. A good example of the type of analysis that can be made from this standpoint is provided by Jo Freeman's article on women's liberation.

The nine chapters of *The Dynamics of Social Movements* are revised versions of papers read by 11 eminent students of the subject in a 1977 symposium at Vanderbilt University. Although all the papers address various questions raised by the new approach, the contributors do not necessarily agree with the editors. Some of them—Bruce Fireman, William Gamson, Anthony Oberschall—differ only in emphasis, but others dissent in various ways. The editors' original formulation was critical of earlier approaches that had accounted for the development of social movements largely in terms of the grievances of those who become involved in them, but some of the authors—John Lofland, Gary Marx, Charles Tilly—continue to emphasize the emotional reactions of the participants. Charles Perrow is quite critical of the editors' position on several grounds.

The term "social movement" refers to a broad range of transactions. It is a popular concept, and further investigation may well reveal that several classes of phenomena are involved and that it may not be possible to account for all of them with a single theory. Fortunately a large body of data is already available; because of their spectacular character, numerous case studies as well as historical and journalistic accounts are already at hand. Furthermore, additional studies can be done with relative ease. Even though the shift in perspective proposed in this book may appear slight, it highlights new problems and suggests different ways of formulating old questions. This in turn will call attention to new hypotheses, and in some instances already available data may be transformed into relevant evidence. Thus, it does not matter whether one agrees or disagrees with the specific formulations of the various contributors. Any serious student of social movements will benefit from a careful study of this volume, and it is a welcome addition to the literature.

Poverty in the United Kingdom: A Survey of Household Resources and Standards of Living. By Peter Townsend. Berkeley and Los Angeles: University of California Press, 1979. Pp. 1216. \$37.50 (cloth); \$15.95 (paper).

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Peter Townsend's *Poverty in the United Kingdom* is one of the most exhaustive, if not the most extensive, study of the problem of poverty attempted to date. While we might like to claim that studies conducted on the problems of poverty in the United States are detailed and encompassing, the British tradition of concern for persons with limited resources is second to none. In particular, the *public* discussion of the problems of poverty is unequalled in any other country, including the United States. With a history documented by Charles Booth, Sidney and Beatrice Webb, Seebohm Rowntree, William Beveridge, and, later, Richard Titmuss, any current discussion has a high standard to emulate and a legacy to uphold. Peter Townsend's work is in this tradition and proves, without doubt, to be equal to it.

The book reports the results of a carefully planned and executed inquiry, conducted in 1968-69 by a specially trained survey research team. The team interviewed over 3,000 households (more than 10,000 persons) using a 39-page questionnaire. Townsend's chapter on the research methodology is especially elucidating, in that he recognizes (and confronts) many of the customary criticisms of survey research—problems in sampling, representation, interviewing, response rates, etc. By treating these issues upfront (and quite successfully), he brings a sense of credibility to the work and provides the reader with an opportunity to appreciate the complexity of his undertaking.

With over 1,200 pages of text, including 27 chapters and 13 appendices, the full measure of Townsend's work cannot be comprehended in a single reading. Indeed, the intellectual contribution of this work will probably not be appreciated fully for some years to come. The wealth of detail, the care with which the research was conducted, and the caveats noted by the author will become fully apparent only after repeated reviews and discussion of the research results.

Townsend begins with two chapters discussing the definitions and theories of poverty and how these are related to such concepts as inequality, deprivation, class, and the labor market. He feels strongly (and this may be his major sociological contribution) that current notions of poverty are simplistic, and drastically underestimate what it means to be poor in the modern era. For example, he points out that many current notions of poverty are tied closely to past labor force status or are based on a "necessities" approach, in which the cost of items "necessary" for subsistence

are totaled and the result labeled a "poverty line." He argues strongly that this approach should be rejected as inadequate. Instead, he posits a more "relativistic" notion of poverty; "Poverty . . . is the lack of resources necessary to permit participation in the activities, customs, and diets commonly approved by society" (p. 88). Thus it is not people's inability to purchase a predetermined marketbasket of goods that makes them poor, but their inability to participate in a whole series of activities that many members of the society take for granted. Poverty becomes, not an absolute standard, but a relative level that can vary from time to time or country to country, depending on differences in life-styles and values.

Townsend further states that poverty is more than lack of income. It is the lack of resources to participate in a society—resources that stem from a variety of resource distribution systems operating in the society. Poverty is the result of the operation of these systems and can result either from the operation of a single system (in which certain benefits are available only to certain groups of people) or from the distribution of resources among systems (in which, for example, a society chooses to favor wage and salary systems in private industry over social insurance systems). From the operation of these various systems, the central concepts of inequality and minority group status are derived. Inequality stems from the differential operation of resource systems, and such inequality, in giving rise to the designation of certain individuals and groups as "minorities," becomes part of the functioning of the society itself. Thus economic relationships come to influence strongly the social relationships in a society; Townsend uses the term "structuration" to describe this process.

With this conceptual background, Townsend proceeds to document how his definition of poverty compares with other definitions when it comes to counting families and individuals. Not surprisingly, Townsend's relative standard results in greater numbers of families and individuals being labeled as poor (25% of the households in Britain in 1968–69, for Townsend's definition, compared to less than 8% using a more restricted, needs-based definition of poverty). In the remaining chapters, various areas of particular deprivation are explored—work, housing, environment, as well as the situations of particular groups of "social minorities"—the poorly paid, the old worker, the disabled and long-term sick, the handicapped, one-parent families. In these explorations, Townsend wishes to demonstrate the validity of his concept of poverty—that it can be related to a variety of circumstances of living and not simply to lack of income. While he acknowledges that lack of income may be a central (or in fact, in many cases, the most important) factor in determining poverty, he concludes that even with such centrality, our understanding of poverty is incomplete unless we explore other dimensions.

In these times not only of diminishing resources but of a shifting in the relationships among resource systems, Townsend's analysis seems especially applicable. The problem of poverty is not limited to income, and in fact, over the longer run, it may be the shift in resource distribution systems

that has the most profound effect on American and British society. Townsend's analysis may at least have helped us to begin to be sensitive to the fact that poverty is far more complex than most of us had previously imagined.

National Development and the World System: Educational, Economic, and Political Change, 1950-1970. Edited by John W. Meyer and Michael T. Hannan. Chicago: University of Chicago Press, 1979. Pp. x+334. \$22.00.

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National Development and the World System, edited by John Meyer and Michael Hannan, is the product of a research group united by an impressive data set and a common commitment to panel analysis. The nation-state was the unit of analysis. Features of national education systems, economic development, and political organization were the central variables. Panel analysis, which combines longitudinal and cross-sectional analysis by incorporating lagged dependent variables, allowed the authors to overcome limitations of cross-sectional analysis in studying the effects of the above institutional sectors on each other. Methodological sophistication was exhibited in treating issues of robustness and in utilizing the flexibility of the panel technique.

Underlying this research plan were ontogenetic notions of national development, which take the nation-state as the relevant unit. Three approaches were framed. The "aggregate individual" approach is demographic in treating national characteristics as the aggregation of individual attributes. The "institutional" approach focuses on the normative structure of a nation rather than on the norms of individuals. The "ecological" approach focuses on material, interactional, and relational constraints at intermediary levels between the individual and society: organization and adaptation are central. Regrettably, these approaches were never compared and contrasted systematically, and the data and findings were not very helpful in assessing their relative worth.

Negative findings compelled the research group to consider exogenetic factors behind national development. The most relevant constraints stemmed from the *world system*. Educational expansion, economic development, and the state form of political organization were *not* found to be strongly contingent upon any national characteristics. They were universals of the world system. Properties of the world system and position within the system were recognized as responsible for national outcomes. These outcomes diverged despite increasing structural similarities among nations.

A dilemma emerged. The contribution of the book was to be "the application of this [panel] design to institutional analysis" (p. 6), yet this method and data were not ideal for studying world system properties. The limited time span of the data prohibited focus on variations in world system patterns: the authors could only acknowledge that their findings might be limited to the post-World War II expansion period. More limiting was the dearth of relational data depicting position within the world system—needed when nation-states cease to be the most relevant unit. A "dependence" measure (based on export partner concentration) was constructed for each nation as the resourceful but only effort in this direction.

A sampling of the findings should illustrate the simultaneous excitement and frustration that await the reader. Meyer and others (chap. 3) found the expansion of national education systems to be a "self-generating process, independent of national economic, political, and social characteristics" (p. 45); Dependence and colonial status slightly retarded expansion, yet no spurt in educational expansion occurred after independence when wealth (GNP) is controlled for. François Nielsen and Hannan (chap. 4) specify the "carrying capacity" of educational systems (separating levels) to be a function of wealth and cohort size. They found structural inertia to be lowest for primary education, which was also the most sensitive to cohort size. Unexpectedly, sensitivity to wealth was curvilinear among both rich and poor countries (i.e., it was highest for secondary education—midway on the scale of complexity and unit costs).

Economic development (GNP/population; energy consumption/population; percentage of male work force not in agriculture) was similarly found to be a universal attribute of the 1950–70 world system. Differentials, however, were moderately accounted for by increases in mass education (and not tertiary education) and by state strength (revenue/population), most significantly in underdeveloped countries (chaps. 6 and 7). Dependence hindered development, a result not found in cross-sectional analysis (chap. 8). Jacques Delacroix (chap. 9), however, shows that specialization in raw material production did not hinder economic growth, as most dependency theorists have asserted. In chapter 10, he shows how informational imports (book translations) weakly aided growth and cultural value imports (movies) weakly hindered it.

Centralized states also attained universal legitimacy during the period studied. Strong states exerted greater control over the educational system (chap. 5) and promoted the incorporation of women into the labor force and educational system (chap. 14). Though strong states facilitated economic growth, states were less likely to be strong when dependence was high, when mining countries were excluded (chaps. 7 and 12). Conversely, economic development had a negative effect on the likelihood of centrist regimes (one-party systems), but the best predictor was simply timing—countries that gained independence after 1944 were most likely to be centrist (chap. 11). Using a broader (1870–1970) time frame and the only application of cross-sectional analysis, John Boli-Bennett (chap. 13) found

that constitutional authority expanded independently of state power and that peripheral and semiperipheral countries have "overcompensated in conforming to world ideology" (p. 234) in their national constitutions.

The editors forgo any effort to assess these individually interesting findings against a single coherent framework. In three concluding chapters, Hannan speculatively uses ecology theory to account for the emergence of ethnic nationalism, Christopher Chase-Dunn and Richard Robinson assure us that "not much is really new" (p. 280) in the world system dynamics of 1950-70 (but we should watch for upcoming ceiling effects), and Meyer and Hannan discuss the potential and limitations of states as strategists, especially with regard to labor markets, in the world system. The editors also do not discuss methodological ramifications stemming from the problem over the relevant unit of analysis—panel analysis with states as units is not questioned. Yet it was not clear how patterns and positions in the world system, which was the theoretical preoccupation of the editors, could be treated by this method in a manner that could yield positive results.

These exclusions will frustrate the reader. However, the authors' extensive data set, their expertise in measurement and analysis, and their abilities to raise important theoretical issues create the high expectations that make this frustration possible. A lot of insight and effort went into this research project. The obstacles encountered—openly and honestly acknowledged by the authors—will undoubtedly provide starting points for future research in the area of national development.

Overshoot: The Ecological Basis of Revolutionary Change. By William R. Catton, Jr. Urbana: University of Illinois Press, 1980. Pp. xx+298. \$16.50.

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The meaning of the title, *Overshoot: The Ecological Basis of Revolutionary Change*, may not be immediately clear to a sociological audience, so some clarification is in order. The expression "revolutionary change" in the subtitle has little or no relation to what one might think of as social or political revolutions. Instead, it seems to refer to transitions between broad stages in the history of the human population's relationship with its environment, such as the hunting-and-gathering, agricultural, and industrial stages, and especially the transition to the future stage of total collapse predicted by William Catton. "Overshoot" is defined in an ecological perspective as an increase in population numbers such that "the habitat's carrying capacity is exceeded by the ecological load" (p. 78). With proper exegesis, the title well captures the main theme of the book. Catton argues that the human species has already far exceeded the earth's carrying capacity, that is, the maximum population that the planet can support indefinitely.

Under these circumstances, a "crash," or decrease in population by various mechanisms, is unavoidable. The crash will lead to revolutionary changes in life-style and loss of many amenities individuals in advanced societies take for granted.

One distinguishing feature of the book is that the author, to make his point, introduces a number of classical ecological concepts and mechanisms: carrying capacity, biomass, ecological succession, climax community, competition, limiting factor, niche diversification, etc. These detailed concepts constitute the backbone of the argument in various chapters. For example, the dependence of our industrial societies on exhaustible supplies of fossil fuels means that humanity is enjoying a "phantom" carrying capacity that allows it to expand temporarily far beyond the numbers the environment can sustain permanently. When these energy resources are exhausted and pollution further reduces the carrying capacity of the environment, the resulting crash will reduce these numbers dramatically. Catton argues that human beings are by no means unique among animal species and cannot escape the consequences of fundamental ecological laws to which all species are subjected. The unavoidable population crash resulting from the "drawdown" of nonrenewable energy resources is just a variation on the general process of ecological succession. The ultimate outcome is the same as for a population of yeast cells feeding on a vat of freshly pressed grape juice. The large supply of sugar produces exuberant growth until this resource is exhausted and the concentration of alcohol (pollution) it produces becomes too great for the yeasts' survival. The population then becomes extinct. There is certainly merit in Catton's effort to apply serious ecological reasoning to the popular context of "environmental concerns" in which intellectual rigor is often lacking, such as in the discourses justifying various philosophical choices that range from a preference for brown sugar to a distaste for nuclear reactors. At the same time, the technical level of Catton's ecological reasoning is not very high, so that the book could be useful for undergraduates, especially since it introduces important ecological concepts in the context of issues that are indeed important, current, and "relevant." It is the kind of book that should generate class discussion.

Another notable aspect of Catton's book is that it is almost strictly gloomy. Unlike others who have written on this topic, he does not propose any comprehensive program to spare humanity a terrible fate. Catton believes that the human species has already overshoot its carrying capacity and that there is not much that can be done to avoid the inevitable crash. The quiet urgings for "ecological modesty" in the last chapter appear almost as an afterthought. The reader is therefore spared many of the vehement recommendations issued by other prophets of doom: no spiritual mortification is urged here, nor are there suggestions that starting a compost pile for your organic garden will prevent catastrophe. The crash is unavoidable. Although this logical possibility is never mentioned in the text, the only group that could view Catton's book as justifying a particu-

lar line of conduct is the survivalists. Since the crash will certainly entail considerable social disorganization and increased competition for a dwindling supply of resources among individuals, until a considerable fraction of the overabundant population is eliminated, the sensible thing to do is prepare by hoarding supplies and weapons to defend oneself and one's family. However, Catton is not totally successful in compelling the reader to join in his radical pessimism. He criticizes arguments by those who believe in a better fate for humanity, first by labeling them as "cargo cults," then by dismissing them one by one in chapter 11. This series of indictments is rather quick and unconvincing. In dismissing nuclear energy, for example (because the problem of radioactive waste has not been resolved), or the prospect of fusion (because ways to contain the overheated plasma have not yet been found), Catton commits a classical historicist fallacy of the type Karl Popper once criticized. Since the future depends on scientific development, which by definition cannot be predicted, no prediction of future social events can be made safely.

The issues addressed by Catton are, in a general sense, important for sociologists since we are, after all, humans interested in the fate of this planet and would have to discard a lot of theorizing about development and societal evolution in general if the scenario described in *Overshoot* takes place. Of more specialized interest for sociologists are the intriguing allusions Catton makes throughout the book to the possibility of a causal relationship between the ecological antagonism due to crowding and various modes of social antagonisms. He implies that a variety of social ills such as nazism, social inequalities, racism, civil and foreign wars, and student unrest are all the result of ecological competition caused by population pressure in an environment with too few resources. Unfortunately this sweeping thesis is only illustrated by suggestive examples, without a detailed demonstration of the mechanisms at work in the translation from ecological to social antagonism. His argument therefore remains unconvincing, although one could imagine a much more serious investigation of such processes, similar to the studies of rat behavior under overcrowded conditions or Gaston Bouthol's argument that the best predictor of war is a surplus in the number of young males (see *La Guerre* by Gaston Bouthol [Paris: Presses Universitaires de France, 1969]).

To summarize, Catton does a good job of generating anxiety about the future of our species, and he does this using serious ecological concepts which are generally useful. However, he dismisses too quickly the role of future scientific advances. There is also merit in his insistence on the fact that humans, just like any other animal species, are subject to fundamental ecological laws. But from the point of view of a professional sociologist many crucial issues such as the relationships between ecological antagonism and the occurrence of social unrest are left undeveloped because of the author's hand-wringing style and sketchy illustrations.

Residential Crowding in Urban America. By Mark Baldassare. Berkeley and Los Angeles: University of California Press, 1979. Pp. xiv+250. \$12.95.

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The effect of crowding on human conduct is one of the most investigated aspects of urban life. It is also an area of inquiry muddled by contradictory findings and disagreements about appropriate methodological and theoretical approaches. Mark Baldassare's book, *Residential Crowding in Urban America*, is a comprehensive, thoughtful, well-written, and generally tightly reasoned effort to clear the air on this important topic. His use of secondary data is imaginative in places and brings an instructive sophistication to some longstanding topics and disputed issues.

Chapter 1 surveys the various literatures on crowding, ranging from the views of classical 19th-century theorists to those expressed in the popular literature and in debates among contemporary urban planners and academics. The opposing views of Philip Hauser and Amos Hawley—the first arguing a connection between density and urban chaos, the second citing the virtues of density—exemplify the confusion surrounding this central problem in urbanology. The book's second chapter cites the weaknesses of the methodologies used in studies of crowding. Baldassare is especially critical of deterministic views derived from animal laboratory studies. In the absence of compelling sociological and social psychological theories, such studies have dominated thinking about the effects of crowding, fostering the perception that crowding necessarily causes all sorts of human pathologies.

After further critical discussion of field studies, housing surveys, and ecological approaches based on aggregated data, the author carries us to the conclusion that "contextual analysis" is the most appropriate method for evaluating the "direct effects of dense urban environments on the social behavior and attitudes of individuals" (p. 48). By using available data from two sources—the 1976 Quality of Life Study conducted by the Michigan Institute for Social Research and 12 "cycles" of the Continuous National Survey conducted by the National Opinion Research Center—Baldassare is able to combine individual-level data on social patterns and attitudes with objective data on neighborhood environments. This strategy allows him to control for a number of neighborhood and individual characteristics other than crowding (such as urbanization of area and age and income of respondents) that might explain the study's two main classes of dependent variables—reports of satisfaction with dwelling and the quality and quantity of individuals' social relations.

Part 2 of the book (chaps. 3 and 4) presents data on housing environ-

ments and family relations. These chapters rebut a number of stereotypes about crowding. For example, they prove that crowding is actually declining in America, that historically metropolitan households have not been more crowded than nonmetropolitan ones, and that poverty is not the best predictor of household crowding. They also offer several intriguing ideas about the influence of power and status on the control of household space.

Although a rich source of ideas, chapters 3 and 4 are somewhat disappointing, first because available data from the two surveys used in this study simply do not allow a clear testing of certain of the author's key hypotheses; and second because the author too often dismisses weak evidence that is contrary to his expectations while putting forth equally weak evidence as tentative support of his theoretical explanations. When, for example, data show unexpectedly that men are more likely than women to feel dissatisfied with crowded households, the finding is chalked up to methodological problems (men may complain more on surveys than women) and is given relatively little serious attention. Such evidence, in fact, makes sense in terms of findings by social psychologists that females are socialized to occupy and accept smaller spaces than men. In particular, chapter 4 ("Family Relations in High Density Housing") resolves few issues, offers a conclusion that slights some of the data presented in the body of the chapter, and ends with a weak call for further research.

The most exciting chapters in the book are those on residential crowding. Chapter 5 assesses the costs and rewards of neighborhood density. This chapter uncovers and explains the importance of persons' location in the life course as a determinant of their responses to high neighborhood population densities. For example, middle-aged persons with children at home show greatest dissatisfaction with crowded conditions.

I found chapter 6 to be the most interesting theoretically. Working from an exchange perspective, Baldassare introduces the notion of "specialized withdrawal" from unprofitable social contacts. He makes important distinctions among intimate friendships, group affiliations, and casual encounters. Congruent with his hypothesis, the author finds that only casual encounters (those with strangers) are reduced as neighborhood density increases. This line of thinking provides an important specification of the "stimulus overload" explanations of urban social contact, first proposed by Georg Simmel and more recently described by Stanley Milgram. Yet, if urban theorists, including the author of this book, are to capture the full range of city experiences, they must describe and analyze those urban places that foster sociability and contact among strangers. Perhaps the notion of "controlled contact" is a more accurate description of such places than Baldassare's "withdrawal" concept.

Residential Crowding in Urban America demonstrates persuasively that any claim of a simple, linear relationship between crowding and urban behavior ignores the complexities of responses to urban density. The last chapter reaffirms this point and suggests the need for studies of selected

"subgroups" which may respond and adapt differently to the same objective crowding conditions. Viewed as a whole, this book succeeds. It revives interest in an important aspect of city life and provides a coherent theoretical platform that should profitably inform further inquiry.

Population and Technological Change: A Study of Long-Term Trends. By Ester Boserup. Chicago: University of Chicago Press, 1981. Pp.+255. \$17.50.

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The central thesis of *Population and Technological Change* appears to be that substantial population size and moderate to high areal density have been and continue to be two necessary preconditions and also quasi-causal sufficient conditions for technological development.

The author ushers her readers through all the technological developments from prehistory to today, in a review of the demographic conditions presumed or inferred to have preceded and accompanied the progressive shifts from simpler to more complex technologies. Ester Boserup finds that increases in population size and density both precede and accompany newer and more advanced stages of technology. Until size and density attain a certain minimum threshold, the society tends to remain on a technological plateau; once the needed threshold is surpassed technological advance takes place. If there is a relapse in size-density, there is technological regression (e.g., the fall of the Roman Empire). This grand ecological correlation, spanning 10 millennia, is explained by the following hypotheses (which I have extracted and made explicit).

1. *Population pressure.* Rising density and declining ratio of available natural resources to population demands the use of more advanced technology to survive. Human ingenuity has thus far delivered the needed "breakthroughs" by devising new and more sophisticated techniques for earning a livelihood.

2. *Infrastructure.* There is a certain minimum size and density without which more complex technologies cannot operate efficiently. By sustained growth populations attain and develop the size-density needed to borrow the advanced technology from societies that have already developed it.

3. *Transport* is an especially key item in Boserup's infrastructure. A more sophisticated technology invariably requires the movement of materials and products from numerous locations to appropriate sites for processing. When density is low, the per capita cost of transport is exorbitant; population growth relieves this constraint.

4. *Diffusion*. Small low-density populations tend to live in isolation, but with increasing size and density there is more lively interaction among the elements of the population, leading to pooling and exchange of ideas and innovations. Consequently, technological advance is facilitated by density-size.

5. *Urbanization* is also a key element in Boserup's infrastructure. Complex technologies require agglomeration of nonagricultural populations. There cannot exist until rural densities rise to a certain minimum level and create a technological and human surplus that can be freed for non-extractive pursuits.

The grand correlation and validation of these hypotheses is established by two lines of research, with statistics and using economic history.

The *statistical argument* consists of constructing a bivariate matrix with population density (five categories) in the rows and technological development (five categories) in the columns. Data for a number of statistical indicators are tabulated and compared in this matrix. Examples are ratio of pasture to arable land, frequency of cropping agricultural land, type of agricultural system, use of chemical fertilizers, percentage of agricultural land irrigated, yield per hectare, degree of industrialization, and output per worker. In each case, more favorable technological conditions exist with higher densities and least favorable conditions exist at the very low densities. Many ecologists would lift an eyebrow at the crudity of the density measurements. Many econometricians will hold a similar view of the technology index (unweighted averages of *rankings* of nations on per capita energy consumption, telephones per capita, life expectancy at birth, and percentage of literacy). One dimension of the matrix ignores the careful research that has been done to evaluate agricultural and other resource potential. The other dimension ignores the equally careful research done on national accounts, work-force composition, and quality-of-life indicators. The old wine is put back in old bottles with new labels: the industrialized nations of Europe, North America, Japan, Australia, and New Zealand fall in the top technology group, while the poorest nations of Africa plus Laos, Afghanistan, Nepal, Haiti, Yemen, and Bangladesh fall in the most technologically backward group. Latin American nations dominate the second most industrialized group, while the more advanced nations of Asia and Africa are in Groups III and IV. Correlations computed from this matrix would probably be lower than those previously computed for all of these variables using more sophisticated measures of the matrix definers. Arriving at validation of the hypotheses from the statistical exercise is more an act of faith than of conventional statistical inference.

The *historical argument* is a formidable review of economic history, agricultural technology, and archaeological economies covering all continents and all of prehistory and history. Readers who (like myself) know little of agricultural ancient history will be impressed with the array of sources and topics discussed with authority. Acceptance of the hypoth-

esis that transitions from long-fallow (slash and burn) to short-fallow, and from short-fallow to annual cropping that took place between 5000 B.C. and A.D. 1500 and, subsequently, the Industrial Revolution were "caused" by the grand ecological correlation and thus validate the five hypotheses stated above also requires some faith if one is concerned about fiducial intervals.

The last section of the book applies the correlation and its corollaries to the contemporary Third World. The author does not make (but does not repudiate) the obvious recommendation implicit in her theory: The poorest nations of the world, such as Bangladesh, Haiti, Senegal, Mauritania, etc., should continue full speed ahead with their population growth until size and density generate the needed pressure to force innovation, the needed urban infrastructure to support modern industries, the population per kilometer of highway demanded for cost-effective transport, etc. Instead, she seeks to explain why there are a number of very densely settled Third World countries with low technological development. Obvious exceptions to her theory are China, India, Bangladesh, Haiti, Pakistan, Egypt, Indonesia, Ghana, Uganda, and North Vietnam (which together constitute more than three-fourths of today's Third World population). The explanation she adduces is colonialism. She concludes, "After all . . . in most of the countries which recently became independent the ruling class is handicapped by insufficient experience; all or nearly all of its members were denied responsible jobs and high level education in the colonial period. Under these circumstances they could hardly be expected to be able to solve the problems of rapid population growth and avoid economic and political difficulties" (p. 211).

It is a pleasant and informative experience to make this fly-by of all human history, viewing the panorama of past economics and demography through the rose-colored windows of Boserup's time capsule. On returning to the unpleasant realities of today's earth, many of her passengers will ungratefully assert that the solutions to the task of trying to raise the quality of life for the poverty-stricken low-density populations currently inhabiting those areas of the planet that, relative to today's technology, are wastelands, are more likely to be found by careful and minute econometric, sociological, and physical science research and evaluation of various projects and experiments than by employing implicit pronatalist folk remedies.

Perhaps the next technological breakthrough will consist of achieving prosperity *despite* low density and conditions of extremely unfavorable resources, a breakthrough in which slow or zero population growth will be an essential ingredient. It could also include massive merging of areal units, with much out-migration and evacuation to more propitious sites. Contemporary Canada, Siberia, and Scandinavia may be more appropriate role models than 14th-century England and France.

Revolution and Tradition in Tientsin, 1949-1952. By Kenneth G. Lieberthal. Stanford, Calif.: Stanford University Press, 1980. Pp. xiv+231. \$18.50.

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Revolution and Tradition in Tientsin, 1949-1952 is a trim and intelligent study of Chinese local politics which, in less than 200 tidy pages of text, outlines the process by which the new government developed its control of one of the regional cities which make up the complex whole of North China. Although he neither evokes the historical sweat and dust which the process must have involved nor wrestles with a theoretical scheme which would scale the sociological empyrean, Kenneth G. Lieberthal, as a political scientist, reports in clear prose the nature and outcome of the political process in the years of initial takeover, during the Korean War, and during what he aptly calls "the second revolution," that is, the turn, by 1952, toward more radical mobilization. At various points, the study also touches on the question of political consciousness and its social structure, as well as the problem of how a strong government can actually control an economy without destroying its chances for development. All in all, this is a neat and almost excessively tight survey, of interest mainly to readers who specialize in the topics covered.

Liu Shao-ch'i (in the Wade-Giles system used in the book, which in the People's Republic would appear as Liu Shaoqi) is the (largely absent) hero of the study. As the master of the underground network in North China before Liberation, Liu must have been familiar with Tientsin and was in general control of the Party there. Yet, when the Communist troops and organizers took over the city, their first tactic was the direct mobilization of the people without adequate organization. Liu's study visit in April and May of 1949 changed this. His "from-the-top-down," organization-centered strategy called for careful expansion of control through the elimination of opposition and the step-by-step training of cadres to enter individual units; giving priority to large-scale and economically advanced institutions; the deferral of socially radical programs of economic or political equality; and seeking the cooperation of Western-oriented businessmen and intellectuals, at least until they could be individually won over or neutralized. One of the problems with local studies is that basic strategy is set on higher levels, leaving the student of the local scene to wonder what the elements of the central decision were and to what extent the local leaders carried out, and to what extent they modified, central directives. This study is no exception. Lieberthal nicely explicates the unfolding of this "top-down" strategy in Tientsin, but he cannot tell who on the local scene might have favored or opposed it, or indeed present us with any local individuals at all—even the Tientsin mayor, who is also First Party Secretary, makes a mere fleeting appearance.

This phase, once launched on its canonical Liuist course, appears much more involved with establishment of the control than with development, or modernization, or revolution—whichever of these vague but parallel terms one feels called upon to use at the moment. We have no particular reason to believe that Tientsin society in its immediately pre-Liberation form was very old; more likely, its functions at that point were a response to the political decay of the Republican period. It is not clear to me that it is proper to analyze the struggle for control in terms of “tradition” and “revolution.” Likewise, one wonders whether the business practices of this thriving comprador port can be categorized meaningfully as traditional. Moreover, Lieberthal seems to show that the process of controlling the economy put great and irrational controls on management.

The top-down strategy, however, could not continue undialectically. The patriotic fervor of the Korean War, the kinks that developed when capitalists and intellectuals were expected to build socialism, and the very success of the Liuist strategy in building a strong control web all led to what Lieberthal calls “the second revolution.” The resulting policy turn, the Three Anti campaign, began with an attack on bureaucratic corruption but soon widened to include, in the form of a Five Anti campaign, an attack on capitalists in general as the source of that corruption. The wave of thought reform completed the turn away from party ties with intellectual, commercial, and industrial interests toward mass mobilization for revolutionary purity.

The conclusion elegantly lays out the competing virtues of the mobilization and the top-down organization strategies and concludes that in 1952 mobilization campaigns served a useful function. By breaking the dependence of the organization on corruptible cadres and on those leaders of bourgeois background who had the skills to run the organization strategy, they extended the organization to new groups and generally shook up the organization’s vested interest in the status quo. However, this rebellion was as circumscribed as that of the little boy who wanted to run away from home but knew that his parents would not let him off the block. Lieberthal argues persuasively that the mobilization strategy works best when it is not taken to its extreme but prepared for by organizational development and focused at specific targets. What worked in 1952, albeit with a price paid in loss of control, could not be repeated in 1958 or 1966. “For all these reasons,” the author (along with certain parties in Peking) concludes, “a strategy of relatively steady change under the firm control of the CCP and its organizations is probably the most effective way to change Chinese society fundamentally over the long run” (p. 190). Thus Lieberthal appears to take “rationalization,” apparently in a general Weberian sense, to be equivalent or parallel to revolution (that is, as opposed to tradition), but he does not go on to use Weber’s distinction between procedural rationality and substantive rationality. A little anguish over the insolubility of the modern human condition might help us all to understand the Chinese Revolution better.

Corporate Power and Urban Crisis in Detroit. By Lynda Ann Ewen. Princeton, N.J.: Princeton University Press, 1978. Pp. xii+312. \$17.50.

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Lynda Ann Ewen's purpose in *Corporate Power and Urban Crisis in Detroit* is to elucidate the connections between the structure of corporate power and the urban crisis in Detroit. Ewen's central thesis is that the social ills of contemporary Detroit are best understood as an outgrowth of the class relations which have given form to the city's history—class relations which by systematically preventing the just and humane allocation of resources created by progressive development in the forces of production have thereby necessitated the development of urban structures of repressive social control.

Ewen opens the book with a description of the syndrome of social problems that make up the urban crisis in Detroit (and everywhere else): poverty, welfare, unemployment, environmental degradation, housing blight, poor working conditions, and crime. In the next three chapters she turns to a brief but informative analysis of the historical development of the propertied and working classes in the Detroit region. Ewen documents how land grants, speculation, and market manipulation created the original Detroit fortunes. Through an elaborate kinship analysis, she then depicts how these early fortunes are connected, through successive generations of inheritance and intermarriage, to power and wealth in Detroit to this very day.

The concentration and centralization of landed wealth in the hands of a propertied class caused the uprooting of small farmers and independent artisans from the land—a process that underpinned the development of the industrial working class in Detroit. Here Ewen offers a statistical portrait of the transfer of labor from agriculture and trade to manufacturing and provides a brief overview of the historical development of working-class organization in Detroit from the mid-19th century through the time of the International Workers of the World to the late 1930s and the beginnings of unionization in the auto industry. Then, in a focus on the development of the black sector of Detroit's working class, Ewen traces the employer "divide and conquer" strategy and the job segregation, poor housing, low-paying jobs, and bad working conditions faced by southern black migrants to the city during the pre- and post-World War II periods.

The next three chapters, which constitute the core of Ewen's study, contain a power structure analysis centering upon the structural relationships among the 41 largest firms with headquarters in the Detroit area. Power relations are traced through the interaction patterns, ownership holdings, and kinship networks that tie together the 421 Detroit area residents who hold directorships in the region's largest firms. The author's major findings

are: (1) taken together, these 41 corporations account for one-third of all nongovernmental employment in the Detroit region; (2) a complex, highly integrated network of intercorporate relations brings representatives from over three-quarters of these firms together in seven corporate boardrooms; (3) this corporate interlock network centers upon the city's major banks and utilities (93% of all interlocking directors hold positions in one of eight banks or utilities); (4) there are identifiable centers of director ownership and control in 26 of the 41 firms; (5) intertwined family fortunes lie behind the holdings of many individual corporate directors (54 directors were related to at least one other director and 26 directors were part of one extended kin group); (6) extensive director membership in prestigious social clubs provides a further basis for class cohesion; and (7) a relatively integrated web of interlocks between the major corporations and Detroit's principal cultural and civic organizations provides the region's corporate capitalist class with the structural means for wielding ideological influence.

Having provided a historical overview of the evolution of class relations in Detroit and an analysis of the contemporary corporate power structure in the region, Ewen takes up the issue of the "ruling class response to urban problems" in Detroit since World War II. Rational urban planning, class integration, and racial and ethnic group assimilation are undermined, the author argues, by the logic of private profit governing the production and allocation of the region's resources. Divorced from the broader community and constrained within the "limits of adaptability of the prevailing business order," city planners create public blueprints for private purposes. Urban planning and urban development signify no more than the spatial rearrangement of a nonetheless persistently intact mosaic of wealth and poverty, luxury consumption and destitution, development and underdevelopment all reflecting Detroit's underlying class relations of wealth and power.

Why, Ewen then queries, has "the working class—the overwhelming majority in the urban area—failed to protect its class interest and to project social plans for its own benefit?" (p. 249). Because, Ewen's parting analysis suggests, a highly cohesive and well-organized corporate elite has successfully wielded its class power to coopt trade union leadership and to divide internally and manipulate to its own purposes the activities of Detroit's working-class organizations. As the economic crisis deepens, as urban contradictions become more glaring, the only viable alternative to the implementation of more repressive political controls by those who now govern Detroit is, the author concludes, a working-class "united front" based on a political party that transcends trade unionism.

The principal strength of this book rests with Ewen's analysis of a corporate power structure in an urban setting. She makes an elaborate investigation of corporate interlocks, financial holdings, kinship networks, and patterns of interaction in social clubs, civic, and cultural organizations. This study contributes much to our understanding of the compo-

sition, structure, and activity of the corporate capitalist class in Detroit and provides a model for researchers wishing to pursue similar investigations in large urban settings.

The principal weakness of this book rests with Ewen's failure to analyze and clarify systematically the nature of the connections between the structure of corporate power and the *urban* crisis in the Detroit region. For Ewen, Detroit is principally a *setting* for a power structure investigation rather than an object of investigation in its own right. Consequently, the specificity of urban questions is lost from view, weakening the author's ability to explain the underlying dynamic and substantive contours of uneven development and social crisis in the Detroit region.

A good book challenges readers to transcend its limitations. To be viewed as an agenda for further research, Ewen's analysis must be supplemented by the following lines of investigation.

1. How ownership of land and control over its use, which created the original fortunes in Detroit, continue to shape patterns of regional development and underdevelopment through the activities of that fraction of the capitalist class (financial, utility, land development, and construction interests) with specific class interests in the content and contours of urban development and how these "property capitalists" are related to the corporate capitalist class as a whole. For example, one important reason banks are at the center of interlocking corporate networks in Detroit is that they hold mortgages on much of the city's land and buildings, and, unlike those of their industrial counterparts, their interests are directly tied to the future of the region because the spatial boundaries of their operations are limited by law. Similarly, utilities are responsible for the construction of urban infrastructure and therefore are intimately involved in urban and regional planning.

2. How major corporations interact with a more extensive power structure to shape the process of urban development. This power structure includes city and regional "consensus seeking organizations," joint public-private urban development corporations, and city, state, and federal government agencies.

3. How the role of urban government is related to this urban development power structure. This role cannot be viewed as a simple reflection of the instrumental interests of corporate capitalists but must be seen as embodying and responding to class antagonisms stemming from the contradictions between the logic of private profit and unmet social needs.

4. How specifically urban class conflicts—community-based struggles surrounding corporate disinvestment, urban renewal, urban service provision, and the like—relate to the structure of urban class relations and urban governance and have, in turn, helped to determine patterns of uneven development and social crisis in the Detroit region.

The Old Social Classes and the Revolutionary Movements of Iraq: A Study of Iraq's Old Landed and Commercial Classes and of Its Communists, Ba'athists, and Free Officers. By Hanna Batatu. Princeton, N.J.: Princeton University Press, 1979. Pp. xxiv+1283. \$85.00.

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Though Hanna Batatu speaks, with great modesty, of the "tentative nature of the present inquiry" (p. 5), this massive volume of nearly 1300 pages is, in fact, a work of monumental scholarship. Not only does Batatu, with characteristic lack of dogmatism and with analytical flexibility, use concepts of class and status group to throw light on a century of socio-political change in Iraq, but he treats major themes of macrosociology judiciously and gives them accurate weight as factors that contributed to social transformation in Iraq.

Book 1 of *The Old Social Classes and the Revolutionary Movements of Iraq* is devoted to the treatment of the "old social classes" who dominated Iraqi society before the revolution of July 1958. In this discussion Batatu finds the process of crystallization of a "class in itself" into a politically conscious social entity, a "class for itself," to be a very complex one. He therefore makes the distinction between a dynamic and a passive class feeling of consciousness (p. 8). This distinction is adhered to implicitly throughout this book.

The essential features of the premodern society in Iraq of the Mamluk period (1749-1831) are not unfamiliar to comparative sociologists. Property was not the basis of social stratification, and political power was as unstable as property (pp. 8-10). Two groups of landlords were most noteworthy: those whose power was derived from their connections with the Ottoman Porte, and those who possessed "largely autonomous tribal or tribally-based power" (p. 66). With the Ottoman reforms of 1858 and, decades later, the land law of 1932, property became much more secure and assumed greater significance as a basis for social stratification and political power. These laws introduced and consolidated a semiprivate property system. The economic power of the tribal shaikhs as landlords grew and became more secure.

The other side of the coin was the conversion of tribesmen with communal rights to land into mere tenants. This, in the long run, facilitated the decomposition of the tribes. The shaikhs' gain in economic power also entailed "alienating them from the only real source of their power: their tribe." The final result is described as "a new *commercial* shaikhly semi-feudalism" (p. 78).

In short, the tribal shaikhs, the officials with land holdings, and the landed clerical notables were slowly and subtly being transformed into a

class. Nevertheless, considerable cleavage between the tribal shaikhs and other status groups who formed the landowning class persisted to the end of the Second World War.

The clerical notables emerge from Batatu's account as a particularly significant status group. Unlike the officials, the pashas, whose power derived from the state and was unstable, the clerical notables enjoyed a stable local power base with autonomous religious legitimacy. Not surprisingly, therefore, the most important standard bearers of dissent and rebellion in the 19th and early 20th centuries came from this group.

Batatu's treatment of the social classes of the old regime is very impressive. However, on a number of occasions it becomes evident that class analysis, even when used as subtly as he uses it, is not flexible enough to explain certain facts and cannot dispense with additional analytical concepts. For example, Batatu states that many descendants of prominent families who "had declined in wealth but not in prestige" were prominent in nationalist and oppositional movements during the years of British ascendancy (pp. 293, 311). When the persons concerned are mentioned by name, it becomes clear that their prominence can be attributed more plausibly to their membership in professional groups and their senses of vocation as intellectuals (left out of Batatu's analytical framework) than to their class background (highlighted in Batatu's class schema). Their political role in general, as well as their specific political platforms, is more easily intelligible if we look at them as lawyers and intellectuals (pp. 293-306) than if we consider them representatives of a declining social class.

Book 2 is entirely devoted to the rise of the Communist party of Iraq and its activities up to 1955. The rationale for giving one-third of the volume to the development of the Communist party is that it "provided a whole generation of Iraqis with not a few of their categories of thought" (p. 466). Furthermore, among the political groupings under the monarchy, "Communists alone had the true characteristics of an organized political party" (p. 478). Underlying this theoretical justification is Batatu's admiration for the Communists and his great sympathy for their cause.

Book 3 consists of an analysis of the political revolutions of the post-monarchical period, the emergence of the Free Officers who put an end to monarchy in 1958, the rise of the Ba'th (Resurrection/Renascence) party, and the Communist party's vicissitudes. It is a highly detailed treatment of the events of the turbulent 1958-68 decade, but, perhaps, with a less well-delineated overview than historical hindsight makes possible for the earlier period. A concise chapter on 1968-early 1970 completes the volume. A central theme and an important subsidiary one, both already discernible in the earlier pages, become dominant in book 3.

The central theme is best designated by Clifford Geertz's phrase "the integrative revolution," a phrase which deserves to maintain its currency. Under the Ottomans, "Iraq consisted to no little extent of distinct, self-absorbed, feebly interconnected societies" (p. 6).

The contribution of Faisal I, as the head of the successor state created in the Ottoman province, to the process of national integration is sympathetically and fairly assessed, and put in the context of: (1) improved transportation and the drawing of Iraq into the world economy, (2) creation of a viable centralized state through the adoption and learning of British administrative skills, and (3) the great extension of educational facilities and creation of a national educational system which instilled Iraqi patriotism and sympathy for the Arab ideal. In book 3, Batatu returns to the theme to show how national loyalty has remained hazy and Iraqism (more palatable to the Shi'ite religious majority and the Kurdish substantial ethnic minority) has had to vie with the Pan-Arabism of the now dominant Ba'ath party. Here he traces, with great care and meticulous scholarship, "the painful, now gradual, now spasmodic growth of an Iraqi national community" (p. 23). The integration of the metropolitan center with the vigorous provinces, along with the disappearance of the tribal periphery, is underlined as a major process of social change. As is the case with Batatu's class analysis, his focus on national integration has some negative consequences: he pays little or no attention to the centrifugal forces. His most notable omission is the lack of consideration of the Kurdish wars.

The author's ubiquitous subsidiary theme is the persistence and salience of primordial and local ties in politics. It is encountered in the process of national integration, spread of political ideologies, formation of political parties, and, above all, in concerted political action aiming at the seizure and securing of power.

Batatu only alludes to the most recent of the trends affecting the formation of Iraqi society: the increasing autonomy of the state from society resulting from its oil revenue (p. 1116). The trend, though set in the 1950s, was significantly reinforced after 1973, and its consequences are still unfolding. By the end of 1977, one-fifth to one-fourth of the inhabitants of Iraq were dependent directly on government for their livelihood (the proportion being as high as one-third in towns) (p. 1123). Consequently, "the relationship of individuals or groups to property has receded in importance, and control of the apparatus of government has become the determinant of social action more conclusively than ever before" (p. 1127).

Nevertheless, Batatu does not lose sight of the significance of the class factor. In conclusion, he stresses the unmistakable dominance of the men of middle condition over the Iraqi regime and over the upper and middle ranges of the administrative corps (p. 1125).

For its richly detailed analyses and for its remarkable avoidance of gratuitous abstraction and "theorizing," *The Old Social Classes and the Revolutionary Movements of Iraq* deserves to be read not only by area specialists, for whom this recommendation is superfluous, but also by all political sociologists and sociologists interested in social change.

Dependent Development and Industrial Order: An Asian Case Study. By Frederic C. Deyo. New York: Praeger Publishers, 1981. Pp. xiv+138. \$19.95.

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In a world preoccupied with the availability of mineral (particularly petroleum) resources, it is a little-noticed fact that the nations with the highest rates of economic growth in the decades before and after the 1973 oil crisis are the resource-poor states or ministates of East Asia. The success of the so-called Gang of Four (Taiwan, Korea, Hong Kong, and Singapore) in export-oriented, labor-intensive industrialization has received some attention from development economists, but very little work has been done on the social and political institutions into which the new proletariat created by this industrialization is integrated.

Dependent Development and Industrial Order makes a valuable contribution to an understanding of how the government of one of these states, Singapore, has attempted to retain a perceived comparative advantage of a docile and productive work force through the incorporation of unions into the state structure. The book's strong point is its detailed analysis of the changing roles of the institutional actors; the paucity of individual-level data on job commitment and social atomism make Frederic Deyo's arguments about the relation of the worker to the new industrial environment less convincing.

The study of the integration of workers during periods of rapid industrialization has a hoary sociological pedigree. While employers and the state often want to discourage worker solidarity and union activity, they also find that the lack of such horizontal ties can lead to a lack of commitment and productivity. Modern Third World governments often find themselves on the horns of this dilemma, and in nations taking the path of export-oriented, labor-intensive industrialization, the tension between short- and long-term goals is particularly acute. While wages must be held down to attract multinational firms, the skills of the population and their job commitment must be continuously upgraded to keep ahead of the increasing competition from newer entrants (such as the Philippines), who can offer a labor force which accepts lower wages.

Frederic Deyo devotes a preliminary chapter to exploring the history of labor relations in Singapore and concludes that the small average size of firm, ethnic segregation by industry type, and the extensive personal and associational ties between employers and employees (the latter often more recent immigrants) all led to a cohesive, complex, and well-integrated set of employment norms and practices that he calls the "communal economy." Organized labor unions grew out of various associations and guilds but only gained real power after World War II, when they took an active role in anticolonialist agitation. The demobilization of labor unions by the ruling People's Action Party during the 1963-68 period involved the sup-

pression of leftist unions and the transformation of the remaining unions into a "productionist" role through the creation of a state-controlled National Trade Union Congress and an industrial arbitration court.

Drawing on the work of Latin American social theorists, Deyo argues that Singapore resembles Mexico (which he characterizes as an example of popular-authoritarian corporatism) more than the typical South American bureaucratic-authoritarian state. Like Mexico, Singapore has a stable, party-led government which did not need military intervention for the establishment of a new technocratic order. While he points out that the island nation has a politically weak domestic bourgeoisie, he parts company from traditional dependency theorists in asserting that the state-sponsored diversification of industrial ventures has resulted in "a diminution in the political influence of multinational firms over local political elites" (p. 115). In addition, he suggests that the rapid increase in employment which accompanied export-led industrialization has led to greater equality of income.

While Deyo makes a good first cut at summarizing the high points in the history of Singaporean labor relations and the relevant theoretical issues, his arguments require more empirical proof than is supplied here. Insofar as the decisions taken by the People's Action Party leadership had a major impact on labor relations, it would be helpful to have a more explicit analysis of what groups inside and outside of the PAP elite pushed for different policies.

Second, Deyo waffles a bit on whether popular-authoritarian corporatism is the wave of the future in states engaging in world-market-oriented industrialization. While he stresses its usefulness in enhancing control in labor-intensive industrialization and notes how it may prevent or co-opt independent social and political mobilization, he also sees it resulting in a "progressive demoralization of the workforce" (p. 112). One possibility is that the current effectiveness of this form of labor control in Singapore is due to a history of strong ties between labor and management and that it would be less effective in nations without this tradition. Whether Taiwan or Korea will begin to build such syndicalist union structures, or whether Singapore will revert to more overt police control of workers as the communal solidarities decay, remains an interesting question.

The Five Dollar Day: Labor Management and Social Control in the Ford Motor Company, 1908-1921. By Stephen Meyer III. Albany: State University of New York Press, 1981. Pp. x+249. \$34.00 (cloth); \$9.95 (paper).

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The Five Dollar Day is a fine example of the "new" social history and one that will be useful to sociologists of work and to students of the labor

process. Stephen Meyer gives a richly detailed account of the transformation of work and "control" at Ford's Highland Park plant between 1908 and 1921. This transformation is explored in three stages: "the development of a new industrial technology, the personal and social reaction of workers to that technology, and the managerial efforts to overcome worker resistance to the new form of production" (p. 5). In each stage problems of control are crucial to Meyer's analysis.

Between 1910 and 1914 Ford revamped the techniques and organization of manufacture—standardizing its product (the Model T), specializing production processes, adopting automatic machine tools, Taylorizing work tasks, and pioneering progressive production and assembly. These changes reflected the efforts of Ford managers to gain control of production. Doing this meant breaking craft control and also involved designing production to utilize the relatively abundant supply of cheap and tractable immigrants. The technological side of this story is well known; Meyer's contribution is to tell the human side. Ford built the skill of the craftsman into machines and the factory system and substituted mechanical and organizational controls (e.g., automatic machine tools, standard machine ratings, clearly defined tasks and routing) for the traditional, autonomous controls of skilled workers. Thus the recalcitrant craftsman could be replaced by an unskilled immigrant.

In gaining control of production, Ford jeopardized its control of workers. New industrial techniques, Meyer suggests, ran up against the pre-industrial culture of immigrants and the resistance of native workers. Here Meyer distinguishes between individual and collective responses to the new industrial order and between "vaguely conscious" (p. 70) and class-conscious action. Concretely, resistance was expressed in turnover and absenteeism, soldiering, output restriction, militant unionism, and socialism. Though Meyer does not say so, this list clearly represents a scale, from individual to collective actions and from vaguely conscious to class-conscious "resistance."

Sociologists of work will appreciate this effort to show the effects of changing industrial organization on labor politics. Unfortunately, Meyer's evidence gets weaker as he moves up the scale. There is no question that turnover and absenteeism were enormous problems for Ford (and Meyer here, as elsewhere, goes behind the statistics to the roots of these phenomena in the work and culture of Ford employees). On soldiering and output restriction the evidence is fragmentary, and hardly justifies Meyer's interpretation of output restriction as class-conscious guerilla warfare. As for militant unionism and socialism, Meyer finds very little indeed at Ford.

By Meyer's own evidence, then, the problem of control confronting Ford managers was largely one of human efficiency—how to reduce the costs of turnover, absenteeism, and insufficient effort—rather than one of containing class-conscious rebellion against new industrial techniques. Between 1913 and 1914 Ford sought to control workers on two fronts. Within the factory, the principal innovation was the skill-wages classification system.

This job hierarchy offered substantial rewards for efficiency and loyalty; and its administration reduced irritating inequities in wage rates as well as the capricious authority of foremen over employment, pay, and discipline.

But Ford also went outside the factory gates to remold working-class culture in accordance with industrial discipline and efficiency. The five-dollar day provided workers with enormous material incentives for altering their private lives as well as their work behavior. The investigatory activities of the company sociology department included culling details of each employee's marital situation, home conditions, sobriety, thrift, etc. A worker's high income, access to company loans, and, ultimately, his job were contingent on reports of suitably industrious, self-disciplined behavior. Finally, the Americanization program, directed at immigrants, mandated a formal company education in American, middle-class values. Meyer gives a detailed account of the operation and impact of these programs, which, given the basic problems Ford set out to solve, worked well. Turnover and absenteeism declined drastically and productivity increased. More broadly, Ford appears to have succeeded in creating the virtuous work force desired. The typical Ford worker came more and more to speak English, stay sober, and have a bank account, a home mortgage, and a wife. In Meyer's view, however, the success was superficial. Ford workers simply did what they had to in order to earn high wages.

World War I changed the calculus on both sides. Ford found paternalism costly, ineffective, and out of style. Monetary incentives for worker quiescence relative to what could be earned from less fastidious employers declined, and there was more to complain about. The carrot was abandoned for the stick; industrial espionage and the sack replaced the five-dollar day. However, Meyer does not actually prove that, even during the war, Ford workers became a threat to managerial control. To be sure, Detroit-wide militancy and wartime grievances fueled managerial fears of rebellion at Ford. Meyer amply documents this fear but not whether it was justified. The most one can conclude is that the policy of repression was preventative. And the choice of this repressive policy over prewar paternalism seems adequately explained by factors other than working-class rebellion at Ford. As Meyer shows, wartime inflation and increasing competitive equality in the industry put substantial monetary rewards for "good" workers beyond Ford's means. After the war, the labor market began to favor capital, so the problems of turnover and absenteeism declined and the threat of the sack became more effective.

Just as rebellion is never shown to have contributed to the rise of paternalism, so it is not shown to have contributed to its fall. Indeed, causality may have been in the opposite direction. Students of the labor process (e.g., Katherine Stone, Richard Edwards, Michael Burawoy, and Theo Nicols) might conclude from Meyer's evidence that Ford's unusual labor policies before 1917 help explain why, even during the war, Ford workers were "normally quiescent" (p. 187). Without comparable studies of other firms it is hard to verify either interpretation of the link between labor

policies and working-class politics. For that reason, among others, it is to be hoped that the new labor history will continue to produce works of the high quality of Meyer's *The Five Dollar Day*.

Union Organization and Militancy: Conclusions from a Study of the United Mine Workers of America, 1940-1974. By Makoto Takamiya. Konigstein: Verlag Anton Hain, 1978. Pp. xi+250. DM 38 (paper).

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For sociologists of organizations and politics, as well as those interested in labor history, the turbulent history of the United Mine Workers of America is a fascinating case for study. Such widely accepted beliefs as the existence of an iron law of oligarchy, the superior effectiveness of hierarchic forms of control, and the nearly exclusive, though militant, economic focus of American unions seem exemplified in most of the UMW's history. Yet the realities of organizational development are not so easily understood. In *Union Organization and Militancy*, Makoto Takamiya describes some of the variation in UMW organizational structure and tactics during a critical 30-year period and attempts to further our understanding of the relation between the two.

Takamiya's central thesis is simply that organizational characteristics modify the influence of economic conditions on propensity of unions to strike. Through analysis of interviews with former union officials and inspection of union documents, he elaborates this thesis into a new theory of "official" union militancy. Although the results of this elaboration cannot be adjudged a complete success, many insights into intraunion conflicts, interunion rivalry, and union relations with government and industry are provided in the course of the attempt.

Chapter 1 is a sketch of "literature on strike activities" that is impoverished by the neglect of such seminal work as that of Alvin Gouldner (*Wildcat Strike* [New York: Harper & Row, 1965]) and David Snyder (who is at least accorded two sentences, on p. 7) ("Institutional Setting and Industrial Conflict," *American Sociological Review*, vol. 40 [1975]), as well as that classic examination of organization and tactics in the International Typographical Union by Seymour Martin Lipset, Martin Trow, and James Coleman (*Union Democracy* [New York: Doubleday, 1962]). Ironically, after concluding that there is a "lack of sufficient theory" (p. 7), Takamiya mentions *Union Democracy* and even the "Iron Law of Oligarchy," in a chapter on methods, without considering their possible use in "hypothesis generation" (p. 9).

Takamiya has entitled his first analytic chapter "Background: The Industry, the Union and the Miner." The unique characteristics of mining

are illustrated effectively, and we are reminded of the importance of the miners' occupational community in increasing their union attachment. Unfortunately, this largely successful chapter is marred by an overly deterministic framework (their "peculiar characteristics" "*made* miners expect their president to be an extraordinary man" [p. 36; my italics]), and by the description of the union before 1940 in only four pages (pp. 20-24). Bitter internal struggles and violent strikes in these years are mentioned without any attempt to relate them to the central thesis.

Takamiya's actual analysis of union militancy does not begin until Lewis is "at the peak of his power and prestige" (p. 24) and has deprived two-thirds of the locals of their autonomy. Takamiya divides the union's subsequent history into four periods, to describe two more decades of Lewis's rule, one decade with Tony Boyle, and then the first two years of Arnold Miller's reform administration.

The first period is one of "high militancy, charismatic leader and devoted members" (p. 39). Still, unauthorized walkouts and dissent in conventions indicated significant challenges to Lewis. Takamiya's use of only central sources precludes much understanding of these conflicts by the reader, but Takamiya concludes that "member loyalty" and Lewis's need to maintain his charisma amplified the pressure toward militancy generated by members' poverty, internal conflicts, and competition from other unions.

In 1950, Lewis abruptly shifted his bargaining strategy from militancy to cooperation with the mining industry's mechanization and focused on economic issues while neglecting the problem of layoffs. In this period, Takamiya argues, "member gratitude for past achievements" and an effective grievance procedure contributed to the maintenance of labor peace.

In the next period, in his discussion of Boyle's increasingly autocratic and corrupt rule, Takamiya describes cogently the problems autocrats have in evaluating their own performance (pp. 118-21). The failures of secondary leaders to "transmit" member sentiment effectively to top leaders are identified as another key basis for an outbreak of wildcat strikes (pp. 122-24). In spite of the inept secondary leaders, the "information flow" apparently began to immerse Boyle in the later 1960s when he adopted increasingly militant positions (p. 110).

With the election of Arnold Miller, a thorough democratization of the union's formal structure began. The key role of a new educated elite of paid union staffers in this process is noted (pp. 137-38), but no attempt is made to interpret this portentous and ironic fact within the theoretical framework. Here Takamiya argues that internal conflicts, the greater openness of the union to member influence, as well as industry growth led to a high degree of militancy.

Does Makoto Takamiya demonstrate the importance of organizational variables in altering the influence of economic conditions on official union militancy? A high degree of official militancy occurred in two periods of oligarchic control (periods 1 and 3) and in the period of democratic con-

trol (period 4). A high degree of official militancy occurred under a charismatic autocrat (period 1) and under a corrupt isolated autocrat (period 3). When secondary leaders were able to "transmit" member sentiment for strikes to the top leaders, official militancy occurred (period 1), yet when the secondary leaders blocked the "information flow," official militancy still occurred (period 3). But these apparent anomalies in the "effects" of organizational factors do not deter Takamiya from confirming his initial thesis. When a variable identified as an "amplifier" in one period does not account for the degree of militancy in another period, he selects another variable (secondary leaders instead of charisma, "gratitude for past achievements" [pp. 85-89] for member loyalty).

In contrast to Takamiya's difficulties with the organizational explanation, he uses fluctuations in the economic prosperity of the industry to provide an economical and consistent explanation of variation in militancy. In the one period when the industry was failing (the 1950s), union militancy was minimal. When economic conditions improved, official militancy increased (p. 10).

It is not that Takamiya's thesis is unreasonable, unsupportable, or even completely unsupported, however. The weaknesses in the analysis have other sources. First the "exploratory approach" is taken as licensing the author to ignore highly relevant literature and to neglect the critical comparison of findings obtained with the initial thesis. Instead, a "search for conditional variables which determine the range of validity of the theory" is conducted whenever an apparently contradictory finding appears (p. 83). Related to this problem in analysis is a lack of consistency in conceptualization and measurement. For example, miners are described as "strongly emotional," impetuous, or vehement (p. 35) and then as harboring "latent hostility" (p. 59).

Finally, Takamiya's concluding "proposed model" (p. 171) is poorly specified. Wildcat strikes are treated as an exogenous and therefore unexplained "physical" pressure toward "official" militancy, thus ignoring the reciprocal and variable relations between institutionalized and non-institutionalized forms of militancy and weakening the explanation of institutionalized militancy. The interrelations between organizational variables are also neglected. The unexamined assumption that organizational variables play only an intervening role between "pressure toward militancy" and "perceived pressure toward militancy" rather than having any direct influence on militancy or a nonrecursive relation with it is still questionable even at the conclusion of the entire analysis.

Still, *Union Organization and Militancy* is a valuable book. It sheds new light on the UMW, and it provides effective illustrations of many important and provocative propositions, both old and new. It would be a fertile source for class discussion of organizational politics and bureaucratization in courses that first provide a grounding in Michels, Weber, and contemporary work in that tradition. These are impressive accomplishments.

Black Representation and Urban Policy. By Albert K. Karnig and Susan Welch. Chicago: University of Chicago Press, 1981. Pp. xiii+179. \$20.00.

Jeffrey Prager

University of California, Los Angeles

The authors of *Black Representation and Urban Policy* pose and attempt to answer two questions: What factors contribute to increased black candidacy and political representation in major American cities? What is the effect on social policy when blacks are more heavily represented in political office? These questions are investigated through large-scale aggregate data analyses of 264 American cities with populations of over 25,000 that are at least 10% black. This is a thoughtful and careful study, identifying a narrow range of questions for empirical investigation. In this sense, its appeal is limited to those researchers interested in political representation and race. As I shall indicate, however, its findings raise theoretical and policy questions which may recommend it to a broader readership.

With respect to the question of candidacy and black representation, Albert Karnig and Susan Welch hypothesize that particular features of the white population and demographic characteristics of the white community—region, population size, income, percentage foreign born, percentage in educational services—have significant effects on the degree of black candidacy and representation at both the city council and mayoral level. The authors predict the impact, on candidacy and representation, of federal antipoverty efforts and municipal political and election rules, for example, form of city government, degree of partisanship, timing and type of local elections, and council size. Finally, they speculate that resources like socioeconomic and organizational characteristics, population size, and protest resources within the black community will independently influence particular political outcomes. In assessing the impact of these factors on the independent variables, they present first a bivariate analysis and then a multivariate regression analysis in which each set of variables is evaluated simultaneously.

Some significant relationships were found between each cluster of dependent variables and black political candidacy and representation. But none were particularly strong or surprising. Perhaps the most interesting set of findings—at least with respect to the question of policy implications for greater black representation—was the impact of political and election rules on election outcomes. For the election of black mayors, a city characterized by a nonpartisan political system had a small positive effect on black election, and black mayors are found in cities where the mayors are elected directly by the public. Black equity in city councils appears to be more strongly achieved in those cities with larger council size. Those cities which have adopted a form of commission government, wherein each of the elected commissioners sits both as a legislator and as the head of an

executive branch department, have produced a consistent pattern of sharp underrepresentation of blacks on the city councils.

But the strongest and most consistent finding of the study is that black resources, especially the proportionate size of the black population and the educational and occupational level of the black population, contribute most significantly to the election of black mayors and council representatives. In a sense, such a finding is predictable. Yet, when seen in the context of the finding that these play a more important role generally than the characteristics of the white population, one is forced to conclude that old forms of prejudice and discrimination, the traditional explanations for black underrepresentation, can no longer stand as the simple and pat answers to this problem. What this finding suggests, furthermore, is that greater black political representation depends not only on a large black population but also on a black community that continues to make occupational and educational advances. The creation of an urban black political elite, in short, requires a secure and expanding black middle class from which the personnel of the political class are largely drawn and from whom financial and other forms of support derive disproportionately.

The importance of this finding for public policy is underscored when Karnig and Welch address their second question: What is the effect of black elected officials on urban policy? As the authors are careful to admit, their findings are tentative especially because of the acute difficulties in measuring effectiveness. And the strength of their findings is anything but decisive. But their preliminary results suggest that black political representation—especially the election of black mayors—results in increased financial support of health, education, and housing. This heightened attention to social policy is often accompanied by a proportionate reduction in protective service budgets, most especially that of the fire department. In short, these findings suggest that black political representation may very well enhance the life chances of the cities' poor through a policy of resource allocation that benefits the poor disproportionately.

Of course, when contrasted with the influence of federal and state policy and expenditures, urban policy holds significantly less force. The study of the effectiveness of the urban political elite will probably, by definition, be inconclusive because of the overriding impact of national and state policies on the cities' population. Yet, seen in another way, this study raises the important theoretical question of the relationship between the sociopolitical order and the economic one, a distinction most sharply drawn by William Wilson in *The Declining Significance of Race* (Chicago: University of Chicago Press, 1978). As Karnig and Welch suggest, if black resources have a significant impact on who gets elected, and if who gets elected has some effect on how political resources become allocated and distributed, then it follows that the racial conflicts in the sociopolitical order cannot be divorced from the real-life conditions of the vast majority of black and other members of the underclass who remain isolated from the mainstream of economic, political, and social life of the city and the nation. In a limited

sense, black political representation, a partial consequence of sociopolitical factors, redounds on the economic conditions of the underclass and therefore the problem of political representation cannot be separated usefully from the plight of the black poor. This study, then, despite its rather circumscribed focus on two specific political questions suggests a fruitful approach to the study of issues that have profound theoretical and policy-oriented consequences.

Racial Inequality: A Political-Economic Analysis. By Michael Reich. Princeton, N.J.: Princeton University Press, 1981. Pp. xii+345. \$22.50 (cloth); \$6.95 (paper).

Robin M. Williams, Jr.
Cornell University

The central problem addressed in *Racial Inequality* is the persistence of substantial differences in income between whites and blacks in the United States despite decades of antidiscrimination efforts, dramatic changes in civil rights, and increased levels of education, income, and political activity of blacks. Michael Reich points out that although blacks have made both absolute and relative gains since the 1950s large gaps remain and future advances are uncertain. Furthermore many of the recent advances represent either business-cycle effects or such one-shot changes as heavy migration out of the agricultural South. The resulting half-full glass is thus half-empty.

The essential thesis of this work, as stated in the concluding chapter, is: "Despite claims of neoclassical economists, income distribution is determined by the conflictual exercise of power between workers and capitalists, and racism benefits capitalists while hurting most white workers" (p. 310). The path to this conclusion is long and often difficult underfoot, as Reich is careful to point out, because of limitations of data, uncertain linkages between data and concepts, and complex problems of inference and levels of analysis (see p. 309). On the whole, the author travels this path with considerable courage and skill.

The persistence of racial inequality in a mobile society under a mass-production capitalistic system poses a severe problem for analysis. Under neoclassical economic assumptions, if resources are fully mobile and a free market prevails, wage rates reflect the "true value" of a given type and grade of labor. The large and continuing black-white differentials (e.g., in median income of families) therefore represent a most puzzling phenomenon if we find persisting differences between equivalent grades and types of workers. Under strict (Milton Friedman) neoclassicism, labor markets should always clear, and racial inequalities in the long run are "irrational" anomalies. To account for racial *discrimination*, one can posit a "taste for

discrimination," human-capital differences, friction between white and black workers, cartel-like behavior for economic gain by whites, or information costs in personnel transactions. The psychic pain model is found to be severely inadequate; the "white cartel," "friction," and crowding models add illumination but are open to serious criticisms. Difficulties in all these models led to models focused on the rational efforts of employers to minimize personnel costs. The latter formulations are seen as worthy of further research and revisions, but the author's econometric analyses lead him to reject the entire neoclassical kit in favor of a class conflict/racism model.

The empirical analysis begins with an examination of the relationship between racial inequality and the distribution of income among whites in the 50 largest Standard Metropolitan Areas in 1960 and 1970 (chap. 4). Racial inequality is measured by the unadjusted ratio of black to white median family incomes, income distribution by the Gini coefficient, and several income-share percentiles. The major findings are that racial inequality is higher where the white income distribution is more unequal (p. 145); income inequality among whites is greater where residential segregation is greater; and where racial inequality is greater, it is the top 1% of white incomes that is disproportionately large. Reich interprets the regressions in terms of "effects" and concludes that "most white workers lose from racism and that rich whites benefit" (p. 158). This inference actually rests on the inconsistency of the pattern of cross-sectional correlations with neoclassical predictions and on its presumed consistency with the author's model. (I shall return to this point below.) The plausibility of the interpretation is then bolstered (chap. 5) by presentation of a class-conflict scheme which portrays the potential solidarity of workers' abilities to advance their economic interests as weakened by cleavages arising from racial inequalities. A searching critique of neoclassical approaches is a strong feature of this discussion; the alternative "class" model is less strong—partly because of the empirically questionable and weakly supported assumptions it contains (e.g., that "the capitalist class and the working class . . . now, more than ever, dominate the economic life of advanced capitalist countries" [p. 187]). In chapter 6, a historical review (1865–1975) is used to support a class conflict/racism explanation of the postbellum South and of unionization and strikes. Chapter 7 caps the argument by reviewing evidence of relations between racial inequality, wages, unionism, education, and public services (again using SMSA data).

The author is careful to state several limitations of the analysis: it concentrates on blacks and does not necessarily hold for other minorities in the United States, nor is it assumed to fit other national societies; as macro-analysis it has nothing to say directly about the unexamined microprocesses; it is handicapped by important deficiencies in available data and extant conceptualizations. Nevertheless, the generally high level of conceptual and empirical caution sometimes cannot be maintained. Early in the book, it is made clear that an end to racial discrimination does not necessarily imply an end to racial inequality. Much of the discussion, however,

ranges easily from *observed* inequality to *assumed* discrimination. Systematic discrimination is inferred, for example, from consistency of pattern: in recent decades blacks have moved out of the South, have concentrated in metropolitan areas, have increased their schooling and political activity; therefore, their relative income should have increased more than it has (p. 74-75).

This is a valuable book—rich in critiques, data, and ideas. Its faults are extensions of its virtues. The underlying “theory” is so strong that inductive leaps can occur with little warning or qualification (e.g., “inequality” tends to merge with “discrimination” and both sometimes blur into an undefined entity called “racism”). The rigor and clarity of the economic analysis are less evident in the treatment of political and social variables. Correlations sometimes are discussed as if they were measures of change over time: they represent “effects”; a low correlation is a “reduction” or a “fall” (e.g., p. 303). When the data agree with predictions, even low R^2 s and a difference that is just “significant at the 15 percent level” are hailed as support.

A political diagnosis is closely tied to the economic analysis, for the author contends that “the economic basis exists for the creation of a broad interracial class alliance opposing racism in all its forms” (p. 12). He is well aware of the history of defeats of efforts to create just such an alliance, and of the pervasive individualism and narrow interest-group behavior that have beset both trade unionism and working-class politics. Nevertheless, because the analysis indicates that only “large capitalists” and “elite professionals” gain from racial inequality, the book holds out the hope that worker solidarity will yet triumph over the latter-day versions of American Exceptionalism.

Wealth and Poverty. By George Gilder. New York: Basic Books, 1981. Pp. xii+306. \$16.95.

Joe R. Feagin

University of Texas at Austin

Wealth and Poverty has become, according to *Time*, the “bible” of the Ronald Reagan administration; many copies are circulating in the highest political circles in Washington, D.C. Excerpts have appeared in *Harper's*, the *Boston Globe*, and the *Wall Street Journal*. Jacket review comments include those of Harvard sociologist Nathan Glazer, who describes the book as a “remarkable analysis of American social and economic policy that demolishes a host of pieties as to the causes of poverty.” It has been widely reviewed and heralded by social scientists and politicians with such phrases as “civilized and brilliant scholarship,” “promethean in intellectual power,” and a “stunning reformulation.” Yet the book is none of these things. It is

in fact a naive political tract defending privilege, wealth, sexism, and the established ways of American capitalism. Little is presented as concrete empirical support for many of the rather incredible array of assertions that are made about American society. In a brief review I can only point out a few of the many flaws in the analysis.

Take, for example, George Gilder's assertions about racial matters. On page 66 he asserts "that blacks can be elected to virtually any office, particularly in big cities, if they are sufficiently resourceful politicians." This statement is only one in a series of claims that racial discrimination is a myth, that blacks no longer suffer from individual or institutional discrimination. Gilder constantly confuses progress in taking down the first, absolute barriers to black progress with the dismantling of all serious discriminatory barriers. Blacks can *run* for virtually any office but they may not be, and are not, *elected* to any office they seek. Today's black elected officials do not include any of the 100 U.S. senators, nor has a black had any chance of being president of the United States in recent years (or in the near future?). And in spite of numerous civil rights laws (e.g., the Voting Rights Act), blacks make up a rather small proportion of all elected officials. For example, in the seven deep South states covered by the Voting Rights Act blacks are 26% of the population but hold only 5.6% of the 32,000 elective offices. Recent black successes, including a few big city mayoral campaigns, generally have come where blacks made up a majority, or a large proportion, of the registered voters. No matter how "resourceful" a black candidate may be, he or she still faces intentional discrimination of various kinds at the hands of white officials and voters.

Gilder continues in the same vein with assertions that the low incomes of black Americans have little or nothing to do with racial discrimination. His explanation of black income inequality and poverty is lack of "work, family, and faith." Indeed, for Gilder the big threat to progress for black men is the feminist movement, because the economic emancipation of (black) women has brought an emphasis on credentials and qualifications for jobs rather than on aggressiveness and competitiveness.

If this seems confusing, it is—at least, until one understands Gilder's basic and explicit bias in favor of the subordination of women to men in all areas of society. Gilder argues that patriarchal arrangements are inevitable and that attempts (e.g., equal opportunity efforts) to emancipate women create severe problems. Much of what many consider to be the result of discrimination is for Gilder natural and desirable. "It is the greater aggressiveness of men, biologically determined but statistically incalculable, that accounts for much of their earnings superiority" (p. 135). Men rank highest as natural group leaders as well. These biological traits of aggressiveness and leadership explain why men work harder outside the home and compete more aggressively for bureaucratic jobs, which in turn explains the much higher incomes of men.

Gilder explicitly argues that a woman's place is in the home rearing children, where women are not a competitive threat to men.

Gilder's arguments are heavily prescriptive. He wants government social programs to be curtailed and presses this argument at every juncture. For example, in his view AFDC programs are undesirable because they have turned (poor) female affections to the government instead of to the (poor) men they should be supporting. He apparently would restrict AFDC further to correct this situation, although the average payment of \$93 per person monthly under AFDC (1979) would already seem to fit Gilder's prescription for austerity.

Gilder sees the rich as the great benefactors of mankind who use their savings to create jobs, wealth, and progress. Capitalism, even Adam Smith suggested, was based on self-interest and greed. Not so, says Gilder. Capitalism is generosity and altruism in action, and is founded on giving and sensitivity to the needs of others.

Gilder concludes with a plea for (Christian) faith. Regarding the resurgence of religious fundamentalism as a good idea, Gilder pleads for Americans "to have faith, to recover the belief in chance and providence, in the ingenuity of free and God-fearing men" (p. 268).

I expected a sophisticated, thorough, and carefully argued defense of the conservative view of wealth and poverty from this book. It is not that. Instead, it is a chic, breezy defense of class, race, and sex privilege by an author whose previous books (*Naked Nomad*, *Sexual Suicide*) are virtually unknown to the scholarly world. And I suspect this book, too, would have remained unknown if powerful conservative figures had not decided to market it as a modern-day rationalization of reactionary political movements.

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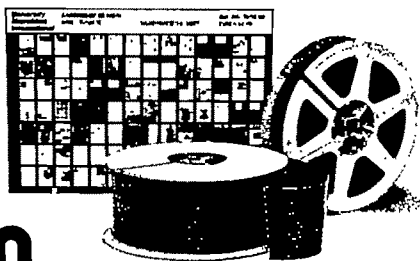
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J. CRAIG JENKINS is associate professor of sociology at the University of Missouri—Columbia. His most recent publications include an article entitled "Resource Mobilization Theory and the Study of Social Movements" and the forthcoming book *The Politics of Insurgency: The Movements of the 1960's and the Farm Worker Movement*.

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VAUGHAN STAPLETON is principal behavioral scientist and director of the Center for the Assessment of the Juvenile Justice System of the American Justice Institute of Sacramento, California. His recent work, at the National Center for State Courts, was as the principal researcher for "A Study of the Structural Characteristics and Operational Procedures of Major Metropolitan Juvenile Courts." He has published several articles on juvenile justice and is coauthor of *In Defense of Youth: A Study of the Role of Counsel in American Juvenile Courts*.

DAVID P. ADAY, JR., is associate professor of sociology at the College of William and Mary in Virginia. He is currently working on methodological issues in survey research and is continuing his research on the structural determinants of juvenile court decision making.

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Why Do Peasants Rebel? Structural and Historical Theories of Modern Peasant Rebellions¹

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Recent analyses of the sources of 20th-century peasant rebellions have centered on two basic theories: a structural theory of class relations that points to the greater political volatility of smallholder tenancy and a historical theory pointing to the strength of traditional village institutions in the midst of the increasing economic insecurity of the peasantry brought about by the expansion of the market economy, increased exactions by landowners and the state, and the pressure of rapid population growth. These theories are evaluated against the experience of the large-scale rebellions that occurred throughout the Russian Empire in 1905–7. Although the historical record provides preliminary support for both theories, regression analysis of the links between provincial socioeconomic patterns and the incidence of the rebellions disconfirms the structural theory and corroborates the basic propositions of the historical theory. Peasants rebel because of threats to their access to an economic subsistence, not because of the particular form of class relations in which they are enmeshed.

One need not accept Frantz Fanon's (1967) assignment of the peasantry to the role of the "revolutionary proletariat of our times" to agree that peasant rebellions have been a crucial political force in the modern world. In fact, the 20th century, opening as it did with major rebellions in Russia, Mexico, and China and witnessing the subsequent spread of peasant uprisings throughout major portions of the world, has legitimately been called the "century of peasant wars" (Wolf 1969). Isolated *jacqueries* were endemic to the historical agrarian empires, but in the 20th century peasant uprisings have assumed sufficient scale and intensity to threaten seriously if not to topple the most powerful of regimes and landed upper classes. Recent interpretations of these modern peasant rebellions have centered on two basic questions. First, what have been the principal social conditions

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giving rise to them? In particular, have specific socioeconomic changes and/or institutional arrangements induced them? Second, which sections or strata of the broad group of primary cultivators conventionally referred to as peasants² have been the chief or leading actors in these rebellions? Has their role been due to the intensity of grievances or to their potential for mobilization?

Two basic theories of modern peasant rebellions have been advanced. One encompasses the structural explanations advanced by Stinchcombe (1961) and Paige (1975) that focus on different systems of class relationships; the other includes the historical explanations of Moore (1966), Wolf (1969), Chirot and Ragin (1975), and Scott (1976) that focus on the increasing economic insecurity of the peasantry engendered by the transition to the modern world. Both theories are species of conflict theory (Collins 1975; Oberschall 1978), asserting that peasant grievances are structurally based on conflicts of interest between peasants and more powerful groups and that the incidence of rebellions is shaped primarily by the intensity of peasant grievances and the potential of peasants for mobilization. The major points of contention center on how these two factors are to be explained.

In the structural explanations, both the intensity of peasant grievances and the potential for mobilization are interpreted as the product of different systems of class relations. In his seminal analysis of agricultural class systems, Stinchcombe (1961) argued that commercial family-sized tenancy is the most volatile system of land tenure. Tenants experience more intense grievances than other lower classes because of the greater sharpness and visibility of their conflicts with landlords. Landlords attempt to extract as much rent as possible and to transfer economic risks to their tenants while reducing their own contributions to production. The relationship approximates a zero-sum situation. Moreover, the marginal productive role of the landlords makes the underlying antagonism highly visible. Tenants view the landlords as dispensable exploiters whose elimination would merely increase their own share of the agricultural product. Tenants also possess a greater capacity for mobilization than other agrarian lower classes. Because the landlords are generally absentee, their social controls over their tenants are weak. Moreover, the tenants typically reside in socially homogeneous villages, an arrangement that generates significant solidarity. In addition, commercial tenancy tends to give rise to a class of independent smallholders who, despite being outside the tenancy system, view the landowners as an obstacle to their upward mobility and therefore

² For our purposes, the "peasantry" consists of rural cultivators who operate small farms with family labor, possess extensive autonomy with regard to productive decisions, and share a heritage of political and economic subordination to a landowning class, a heritage that has given rise to sharp cultural distinctions in terms of language, manners, and morals (cf. Moore 1966, p. 111; Wolf 1969, p. xiv; Shanin 1971, pp. 14-15).

serve as instigators of rebellions. In comparison, other class systems give rise to lesser grievances and weaker bases for lower-class mobilization. Under the manorial, plantation, and ranch systems, the landowners assume a direct role in the management of their estates, reducing the sharpness and visibility of the underlying class conflicts. Moreover, the landlords are engaged in daily social contacts with their workers, frequently extending paternalistic protections that provide a basis for upper-class social control. As for independent smallholders, they are sufficiently disorganized by their ecological dispersion and social isolation on small farms to lack the solidarity to mount sustained rebellions.

In his reformulation of this basic theory, Paige (1975) has offered a more systematic theory of the bases of lower-class grievances and potential for mobilization.³ The intensity of grievances depends primarily on the basis (land vs. capital) of upper-class income, while mobilization around these grievances depends primarily upon the form (in-kind vs. cash) of lower-class income. On both manorial and sharecropping estates, the upper class derives its income from the ownership of land rather than capital. Increases in productivity are relatively small, meaning that the class struggle over the distribution of income is intense, approximating a zero-sum conflict. Peasants advance their interests only by open rebellion, either direct land seizures or support for a revolutionary movement that promises wholesale land reform. In contrast, under plantation and small farmer systems the upper class derives its income from the ownership of capital. Productivity increases are higher, enabling the upper class to grant economic concessions that effectively channel lower-class actions in the direction of reform rather than open rebellions. Rebellions are therefore confined to manorial and sharecropping systems. While lower-class grievances are similar under manorial and sharecropping systems, the potential for mobilization is greater under the latter. On manorial estates, the peasants secure their income from subsistence plots received in exchange for labor on the domain. This arrangement fosters ties to the land that give rise to a risk-averse peasant culture and undermine solidarity. The manorial peasants are highly individualistic, competing for the choice plots, carrying out production on isolated family plots, and pursuing individual mobility. Moreover, the landowners are directly involved in the management of their estates, forging strong paternalistic controls that inhibit rebellions. Uprisings are therefore rare, taking the form of isolated *jacqueries* that become large-scale

³ While Paige seeks primarily to account for the various goals of rural social movements, his book also contains an analysis of the likelihood of rebellions insofar as they are shaped by the intensity of grievances and the solidarity of rural cultivators. Although his analysis of sharecropping differs from Stinchcombe's treatment of tenancy insofar as Paige emphasizes the form of peasant income rather than family control over the land, his discussion (1975, pp. 6-7) makes clear that he views any smallholder tenancy system, regardless of the use of cash or share rents, as the most volatile class system.

rebellions only if the coercive monopoly of the landowners is suddenly destroyed by outside political forces. In comparison, sharecroppers are fully enmeshed in the market economy and are therefore more willing to take risks. Moreover, they possess greater solidarity. Their weak ties to the land reduce the intensity of competition for plots, inhibit the formation of strong paternalistic controls by the landowners, and discourage mobility aspirations. In other words, while Stinchcombe and Paige disagree over the precise factors shaping peasant grievances and mobilization, they agree on the central thesis that smallholder tenants are more likely to rebel than peasants on manorial estates, workers on large plantations and ranches, or small farmers.

In place of this comparative focus on class structures, the proponents of the historical theories have emphasized the social discontinuities created by the transition to the modern world. Broadly speaking, there have been two historical formulations—the “market” theory advanced by Hobsbawm (1959), Wolf (1969), Migdal (1974), and Chirot and Ragin (1975) and the “exploitation” theory advanced by Moore (1966) and Scott (1976). Both versions argue that peasant grievances center on increasing economic insecurity—the market theory by pointing to the sharp fluctuations of a market economy and the exploitation theory to the increasing economic exactions of landowners and the state.

Eric Wolf (1969) has been the major proponent of the thesis that the rapid expansion of the commercial market destroys the economic security of the peasantry, thereby stimulating rebellions. Traditionally peasants have guaranteed their access to an economic subsistence by holding the market at arm's length. Inalienable allotments, the three-field system, the patronage of the large landowners, and various collective insurance measures such as common pasturage and forests and periodic repartition of plowlands have minimized economic risks. The intrusion of the market economy transforms the agrarian economy, destroying these security measures. As Wolf puts it: “Capitalism necessarily produces a revolution of its own” (1969, p. 278). Plowlands are transformed into alienable commodities, purchased on the market rather than available as a social right; commons and forests traditionally open to the villagers are forcibly enclosed by the landowners, available only upon the payment of rent; the landowners abrogate their traditional protective responsibilities, reorganizing their estates to turn the maximum profit; the peasants find themselves forced increasingly into the market, where grain and consumption items are priced according to market principles rather than the “just price.” As Chirot and Ragin summarize the argument, the peasantry is threatened by the “high insecurity of a rural economy in which world prices and the impersonal market determine the availability of land and the actual reward received by peasant producers” (1975, p. 443). The likelihood of rebellion,

then, is greatest during the period in which the traditional economic culture of the peasantry remains intact but is being challenged by the rapid expansion of the market economy. The more rapid, large scale, and thoroughgoing the process of commercialization, the greater the likelihood of rebellion.

The exploitation theory differs by emphasizing the level of the economic exactions imposed on the peasantry and by tracing these to the pressures of state building, the demands of the large landowners, and rapid population growth. In Moore's (1966, pp. 459-68) formulation, agrarian-based bureaucratic empires provided the most likely setting for peasant rebellions. Because the landowners depended on the central authorities to extract an economic surplus, they were unlikely to reorganize their estates as commercial farms despite expanding market opportunities. This had two long-term consequences. The landowners tended to increase their "take" from a relatively fixed base of peasant production rather than a fixed share of an expanding product. At the same time, they tended to lessen their involvement in the peasant villages, reducing their protective functions while leaving the villages sufficiently cohesive and autonomous to serve as a basis for collective action. Moreover, as Skocpol (1979) has argued, bureaucratic empires such as 19th-century Russia have also confronted intense international military pressures from the more developed states that have prompted the state managers to attempt internal economic reforms. Insofar as the landowners have been sufficiently powerful to block or hinder these reforms and retain their privileges, the state has found it imperative to increase its exactions from the peasantry.

Scott (1976) has demonstrated that substantially the same argument can be extended to colonial settings. As long as the basic privileges of the landowners are left intact, the rationalization of the courts and legal enforcement entailed by state building provides the landowners with more reliable enforcement of their demands. At the same time, the construction of a central bureaucratic administration requires increasing taxation enforced with increasing inflexibility. Tax obligations are shifted from the village to the household and taxes are collected with growing disregard for short-term crop losses or the household's ability to pay. Moreover, to increase their claim to "mobile" resources, the state managers find it desirable to promote the participation of the peasantry in the market economy.

These threats to the economic security of the peasantry are reinforced by rapid increases in the rural population, especially where coercive restrictions are imposed on geographic mobility or alternative employment is unavailable. Because the amount of subsistence lands available to the peasants is generally fixed, rapid increases in the rural population reduce the amount of land available to each household and intensify competition among the peasants. The bargaining position of the landowners is thereby strengthened,

increasing their ability to extract an economic surplus. Rents and the price of land are driven upward while wages are depressed. The already tenuous hold of the peasantry on an economic subsistence is further weakened.

Both historical formulations agree that the capacity of the peasants to mobilize against these threats depends largely on the strength of traditional village institutions that generate solidarity and independence from the landowners. Moore (1966, pp. 475–79) and Wolf (1969, p. 291) argue that the solidarity of the village depends on the strength of communal traditions, especially the existence of collective land rights and village-level exchange. As a general rule, the less the commercialization, the greater the degree of solidarity. Households still reside on the village street rather than on isolated homesteads; collective rights to plowlands and pasturage serve as a constant reminder of common interests that clash with those of the landowners; cooperative tasks based on the three-field system and the use of commons foster a sense of collective fate. Moreover, these institutions provide peasants with significant autonomy from the landowners. Peasants who retain access to subsistence plots possess greater tactical independence that can be used against the landowners (Wolf 1969, p. 291). Villages that control local governmental decisions with a minimum of interference from the local landowners and state officials possess a built-in forum for the “creation of a distinct peasant society in opposition to the dominant class and as the basis for popular conceptions of justice and injustice that clash with those of the rulers” (Moore 1966, p. 479; cf. Skocpol 1979, pp. 116, 132). In sum, the rebel peasant is the “middle” peasant who retains claims to subsistence lands and “most depends on traditional social relations of kind and mutual aid between neighbors” (Wolf 1969, p. 292).

The chief points of contention among these theories, then, are the sources of peasant grievances and the basis for mobilization (table 1). For the structural formulations, smallholder tenancy generates both intense grievances and a high potential for mobilization. In a region characterized by diverse systems of land tenure, the incidence of rebellion should be closely linked to the existence of smallholder tenancy and sharecropping. In the historical formulations, peasant grievances stem from long-term social changes. The intrusion of the commercial market, the increasing exactions of landowners and the state, and rapid population growth undermine the traditional claims of the peasantry to economic subsistence. Peasant mobilization, however, hinges on the preservation of traditional village institutions that provide solidarity and independence from the landowners and the state. “Middle” peasants, not tenants, form the backbone of peasant rebellions.

This paper assesses these theories as interpretations of the peasant rebellions that enveloped the Russian Empire in 1905–7. By examining changes in land tenure, market participation, taxes, and village institutions

TABLE 1
STRUCTURAL AND HISTORICAL THEORIES OF PEASANT REBELLIONS

	Structural Theory	Historical Theory
Structural potential for rebellion:		
Intensity of peasant grievances.....	Class system that entails: 1. Zero-sum conflict of interests 2. Highly visible conflicts of interest ...	Increased economic insecurity traced to: 1. Commercialization 2. State-building that reinforces landowner claims and institutes new inflexible claims 3. Rural population growth
Mobilization potential of the peasants.....	Class system that entails: 1. Minimal competition among households 2. Weak patronage ties 3. Few prospects for upward mobility 4. Market exposure and risk-taking culture 5. Proximity of independent smallholders who serve as leaders	Village solidarity and autonomy based on: 1. Minimal economic differentiation 2. Collective land rights 3. Village cooperation 4. Secure claims to subsistence plots 5. Autonomous village government
Identity of rebels.....	Smallholder tenants or sharecroppers	Middle peasants

during the late 19th and early 20th centuries, the analysis traces the shifting structural potential for rebellion. The link between these factors and the rebellions is then assessed by an ecological analysis of the incidence of the uprisings.

THE RUSSIAN PEASANT UNDER THE OLD REGIME

The late 19th century witnessed major changes in rural Russia. Prior to the emancipation of the serfs in 1861, Russian agriculture was overwhelmingly oriented toward subsistence production. Except for the far southwest, where serfdom did not exist and easy transportation provided access to the Western European markets, the bulk of agricultural production did not enter the market. The peasants were tied to the land and obligated to provide two or three days of labor per week on the lord's *demesne* in exchange for access to subsistence plots. The personal authority of the noble landowner was complete and increasingly unchecked. The emancipation of the serfs set the stage for a series of changes that culminated in the rebellions of 1905-7. The majority of the noble landowners turned to tenancy or sold off their estates. The production of grain and "industrial" crops for the market expanded rapidly, especially in the outer provinces of the empire.

Toward the end of the century, the state launched a "hothouse" industrialization program that rested both on an increasing tax levy on the peasantry and on rapidly growing grain exports. At the same time, the peasant population increased, outpacing the increase in peasant lands. In the midst of these economic changes, traditional village institutions remained largely intact. If anything, the villagers became even more independent from the large landowners and the state.

The roots of these changes lay in the traditions of landowner-peasant relations established in the course of the expansion of the Russian Empire and the intensified international challenges confronting the empire in the late 19th century. Beginning with the early expansion of the Muscovite state in the 15th century, the Russian nobility became progressively subordinated to the state, serving in the military and central administration and leaving their estates largely in the hands of the peasants. The principal bases for the building of the empire were the enserfment of the peasantry and the increasing prominence of conditional land tenure (or *pomestia*) over the absolute land rights (or *volchiny*) of the old aristocracy. Enserfment strengthened the claims of the landowning nobles to an economic surplus and reduced the nobles' role in managing their estates (Hellie 1971). In 1682 hereditary rights to office were abolished, preparing the way for the promulgation by Peter I (Peter the Great) in 1722 of a unified "Table of Noble Ranks" that imposed lifetime state service on the nobility as a condition for holding their estates. Though Peter III abolished compulsory state service in 1762, the majority of the nobles lacked sufficient holdings and serfs to derive an adequate income from their estates and continued to rely on state offices (Raef 1966, 1971; p. 96). Because of the continuous rotation of service and the frequent scattering of holdings, the nobles treated their estates as sinecures rather than productive enterprises, frequently residing in the cities and leaving the management of their property in the hands of bailiffs (Beloff 1953; Blum 1961, pp. 386-88; Pipes 1974, pp. 172-79). The landowners' marginal role in their estates created a volatile situation. The peasantry was highly independent, viewing the *pomeshchik* as an "outsider" (Field 1976, p. 22). Moreover, instead of modernizing their estates in response to commercial opportunities, the nobles merely increased their claims on their serfs (Field 1976, pp. 24-32). On the eve of the emancipation, serf labor requirements (or *barshchina*) of three, four, and even five days per week were common throughout the central black earth districts of the empire (Blum 1961, pp. 386-413).

The major losses in the Crimean War of 1854-55 and the wave of peasant uprisings behind the lines shocked Alexander II and his advisers into major reforms designed to strengthen the empire's military position vis-à-vis the Western European powers. Concluding that the military reversals were due to the empire's economic backwardness, especially the weakness of war-

related industries and the obstacles posed by serfdom to the maintenance of a standing army, the tsar resolved to force emancipation of the serfs (Gerschenkron 1962, p. 142; Rieber 1971; Field 1976, pp. 15-25).

The emancipation and the accompanying governmental reforms set the main lines for subsequent economic and institutional changes. In order to avoid the political instability attached to the landless emancipation imposed in the Baltic provinces in 1816-19, the freed peasants were to possess allotments roughly equal to the plots held under serfdom. The nobles were to receive as private property lands equivalent to their former demesnes. They were to be compensated for the loss of their lands but not for the loss of personal control over the peasants. The state assumed responsibility for financing the transactions, paying the nobles outright for their lost estate lands and collecting repayment from the peasants in the form of redemption payments scheduled over a 49-year period (for a general review, see Robinson 1932, chap. 5; Emmons 1968; Field 1976).

If the emancipation was intended to generate a self-sufficient peasant agriculture, noble control over its implementation guaranteed that it did not do so. Provincial councils of the nobility were charged with the task of subdividing the estates and fixing the prices paid for the allotments, thereby insuring the nobles a strong hand in the final land settlement. The terms of the settlement varied from region to region primarily as a function of soil quality. In the north where the soil was poor, the nobles retained the valuable forests and forced the marginal plowlands on the peasants. In the *chernozem*, the fertile black soil belt that ran through the center of European Russia, the nobility expanded their holdings, "cutting off" (reducing) peasant plots by an average of 20%-40%. Frequently the nobles were able to persuade the peasants to accept miniature "pauper plots" roughly one-quarter the size of average allotments. Although such peasants were free of redemption obligations, the size of their plots guaranteed that they would have to depend on their former masters for a livelihood. In addition, the majority of the former household serfs and all those serving in the military were emancipated without land. If the holders of pauper plots and the emancipated landless are combined, roughly one-quarter of the former seignorial serfs began their freedom effectively without land. For the remainder, the size of the land reductions, or "cutoffs" (*otrezki*), and the fact that these were generally the most valuable lands on the estates vitiated the possibility of an autonomous agriculture. Further thwarting the possibility, the provincial councils set the prices for the allotment lands far in excess of their market value, frequently at double or more the prevailing price (Liashchenko 1949, p. 304; Gerschenkron 1968, pp. 167-68; Volin 1970, pp. 44-52).

In the central black earth districts, the land reform set the stage for the development of smallholder tenancy. The cutoffs and the excessive redemp-

tion payments insured that the peasants lacked sufficient lands to meet obligations independently and still produce a subsistence. Moreover, the peasants were effectively tied to the land by their allotments. Obligations for redemption and taxes were fixed on the village rather than the individual household. Without full payment of the redemption price and land taxes and the approval of two-thirds of the villagers, allotments could not be transferred. Nor were the state officials overseeing land transactions receptive to the alienation of allotments. From the state's point of view, this guaranteed the long-term ability to collect the redemption payments and taxes. From that of the nobility, the arrangement insured the availability of an immobile peasant labor force that was cheap because of its "land hunger." Nor were the nobles prepared to reorganize their estates as viable capitalist enterprises. Not only did they lack entrepreneurial experience and the implements to cultivate their estates, but also the one source of capital that might have been employed for the modernization of the estates (the fees for the estate lands) was consumed primarily by repayment of massive outstanding debts to the imperial government (Robinson 1932, p. 129). Especially in the central black earth districts, the nobles turned to various leasing arrangements, restoring ties to their former serfs. Because of the limited development of market institutions, share rents were the dominant practice. By the turn of the century, over 19.5 million of the 75 million *desiatinas* (1 *des.* = 1.6 acres) of noble lands were leased to peasants for use as plowlands. The peasants also leased substantial amounts of pasturage and forests, bringing the total to over 40% of all lands owned by the nobility (Robinson 1932, pp. 99, 268, 290; Anfimov 1962, p. 44; Watters 1968, p. 150). In the empire as a whole, more than one-third of all peasant households leased privately owned lands, chiefly from the nobles in the black earth provinces where the cutoffs had been the most extensive (Pavlovsky [1930] 1968, p. 106; Volin 1970, pp. 73-75).

The emancipation also set the stage for the expansion of the commercial market, promoting market-oriented production and increased market participation by the peasantry. The most striking shift was the rapid increase in the production of cereal grains, rising from 1.5 million poods in 1864 to almost 4 million by 1900. The sharpest increases were in wheat production, which expanded to over one-quarter of the total sown area by the end of the century, and the various "industrial" crops (cotton, flax, hemp, potatoes, and sugarbeets). Over the same period, the cultivation of the chief subsistence staple of the peasantry—rye—remained virtually constant. By the turn of the century, almost one-half of the total grain crop entered the market, almost 90% of all wheat but only 20% of the rye, an expanding share of which was exported to Western Europe (Liashchenko 1949, pp. 445-53, 516-20; Simms 1977, p. 392). This expansion of the market, however, was concentrated in the outer provinces (especially the

southwest and southeast) where large capitalist estates and rail transportation facilitated export production (Liashchenko 1949, pp. 464-66; Symons 1972, pp. 35-42).

That localized markets and subsistence production continued to prevail in the central districts does not mean that the peasants succeeded in holding the market at arm's length. For one thing, the peasants had to dispose of a significant portion of their crops merely to meet redemption and tax obligations which could be paid only in cash. By the 1890s, between one-fourth and one-third of peasant production in the black earth districts ended up in the market (Volin 1970, p. 60). For another, the land-hungry peasants found themselves forced to enter the market to secure sufficient land. Initially, tenancy arrangements were based on labor rents or sharecropping, but, with the steady decline in grain prices after 1890, the noble landowners increasingly demanded cash rents, thereby shifting more of the risks to the peasantry. In addition, the peasants purchased significant amounts of private lands, especially after the 1880s when the nobles began selling off their estates en masse. Between 1861 and 1905, the nobility disposed of over 36 million *desiatinas*, over two-fifths of their total holdings. Of this amount, the peasants purchased over one-half, expanding their private holdings from 5.7 million to 25.4 million *desiatinas* (Oganovskii 1923, pp. 60-61).

For the majority of the peasants, these changes were accompanied by a declining margin of economic security that made tenancy and market participation all the more hazardous. Because of access to improved health care and more regular food supply, the peasant population increased sharply from 61.4 million in 1865 to 107.6 million in 1902. Though this population increase was surpassed by a 125% increase in total grain production (from 1,232 million to 2,735 million poods), little of the additional grain appears to have ended up in the hands of the peasants. The allotment lands, the chief source of peasant subsistence, fell from an average of 13.6 *desiatinas* per household in 1865 to 7.4 in 1905. Nor did the purchase of private land and rentals fully compensate for this decline. The total lands (allotment, private, and rented) under the peasant plow averaged only 12.9 *desiatinas* per household in 1902.⁴ If the increasing payments for these lands are included, the lot of the peasantry appears to have worsened. Because the peasants were tied to specific locales by their redemption debts, the population increase rapidly drove up land prices and rents. Between 1883 and 1903, the average price paid by peasants for private lands doubled and rents increased slightly more, rising the most rapidly on the smallest parcels

⁴ These calculations are based on the figures for population, allotment, and private holdings provided by Oganovskii (1923) and the rental figures of Prokopovich (1924) and Volin (1970, pp. 73-75). For a thorough but, I believe, misleading review of the evidence on the changing economic condition of the peasantry in the late 19th century, see Crisp (1976) and Simms (1977). For an opposing view, see Goldsmith (1961, p. 442) and Gerschenkron (1968).

(Robinson 1932, pp. 99-102; Volin 1970, pp. 70-74). Nor was there a significant increase in peasant productivity, because the major increases in yields were centered on the large capitalist estates of the southwest (Volin 1970, pp. 61-69). In effect, the increase in grain production was largely channeled into exports rather than domestic consumption, an arrangement that strengthened the empire's international credit but left the peasants' plates still empty. Nor did the peasants reap increased rewards from their entry into the market. Because of intensified competition in the international markets after the 1880s, grain prices fell steadily: wheat prices slid downward by almost one-third between 1883 and 1903 (Liashchenko 1949, p. 468).

In the midst of these changes, the imperial government launched a vigorous state-guided industrialization program to strengthen its international military position. Immediately following the emancipation, the government had adopted a *laissez-faire* stance, reducing tariffs and encouraging foreign investments to build an industrial base while relying on grain exports to maintain a balance of trade. Internal reforms remained limited: a cabinet government and rationalized budget; military reorganizations to strengthen the professional staff; and the establishment of the *zemstvo* governments. The peasant "soul" tax remained the major source of revenue (Von Laue 1963, pp. 6-26). But continued agricultural stagnation and declining grain prices coupled with escalating military requirements eventually forced a major shift in policy. Prodded by the widespread peasant famines in 1891, the government finally embarked on the "Witte system": extensive expenditures on a state-owned railway system, high protective tariffs, licensed cartels and heavy subsidies to stimulate Russian industry, a wholesale restructuring of the tax system, and more vigorous promotion of grain exports to provide a balance of trade and monetary stability (Von Laue 1963, pp. 26-36; 1968).

The peasantry felt the shift in two ways—increased levies against peasant consumption stemming from rising taxes and tariffs, and increased involvement in the export grain market. The two, of course, were essential parts of the industrialization program. Increased tax revenues and the expansion of grain exports were essential if the government was to invest in heavy industry while simultaneously building sufficient gold reserves to place the ruble on the gold standard. The convertibility of the ruble was, in turn, essential if the government was to secure the required foreign loans, encourage foreign investments, and purchase Western technology. The policies proved equally interrelated for the peasant. Increased taxes and rising prices for industrial goods meant that peasants had to market an increasing share of their grain. Of that marketed grain, an increasing portion found its way into the export market.

By all measures, the program was a remarkable success. Between 1890

and 1901, the tax revenues of the imperial government increased sixfold, the rates alone rising by 108% for indirect taxes and 80% for direct taxes. State monopolies were established on items of general consumption (sugar, matches, tobacco, kerosene, and spirits), creating a major drain on peasant incomes. Though the soul tax was abolished in 1885, the government more than compensated by sharply increasing the land taxes imposed on the peasantry. In addition, the government installed heavy import tariffs on industrial goods, generating additional revenues and providing a protected market for domestic industry. Further, toward the end of the century the imperial government renewed its efforts to collect arrears for the abolished soul tax and the lagging redemption payments (Von Laue 1963, pp. 26–28; Liashchenko 1949, pp. 556–57; Seton-Watson 1963, p. 50). On the eve of the rebellions, the total economic surplus extracted from the peasantry through taxation was conservatively estimated at one-fifth of peasant production (Shanin 1972:22). Equally impressive was the increase in grain exports. Between 1861–65 and 1901–5, the value of grain exports rose from 56.3 million rubles to over 447 million and the volume from slightly under 80 poods to over 444 million, the most dramatic increase taking place in the 1890s (Liashchenko 1949, pp. 518–21, 737). Despite the sharp decline in grain prices, the growth in exports was sufficient to generate a favorable trade balance, and in 1898 the government finally instituted the gold standard. The result was a remarkable upsurge in industrial production. Between 1891 and 1905, the rate of industrial growth averaged 8%, creating a significant base of heavy industry and an extensive railway system (Carson 1959, pp. 116–27).

The combination of these trends—increasing tenancy, the expansion of the market, population growth, and rising state exactions—rendered the position of the peasantry increasingly precarious. As if to verify the impact of the emancipation and the industrialization program, the imperial government conducted a series of studies of the “peasant problem.” In their feverish attempt to keep pace with mounting obligations and population pressure, the peasants were exhausting the soil, abandoning the principal benefit of the traditional three-field system still in practice—namely, leaving the land fallow every third year to replenish fertility. Nor were the peasants investing in fertilizers, either the new chemical varieties or the traditional animal sources. Instead they were bringing vital pasturage and forests under the plow in a desperate bid to expand grain production (Liashchenko 1949, p. 450). The major form of peasant wealth—horses and cattle—was rapidly being depleted, leaving a large and increasing proportion of the households without the aid of work animals (Liashchenko 1949, pp. 732–33). And though a growing number of peasants found economic refuge in “go-away” work in the cities and the traditional *kustar* industries, these options

were primarily confined to the peasants in the northern industrial provinces (Robinson 1932, pp. 104-10).

A rough gauge of how hard-pressed the peasants found themselves is given by the striking increase in tax arrears that accumulated at the end of the 19th century. By 1894, over half of all peasant households in the *chernozem* were in arrears on their land taxes or redemption payments. By 1900, the arrears on the land tax were over 100% of the annual levy and, in several of the black earth provinces, arrears on redemption payments ranged as high as 400% of the annual dues (Wallace 1912, pp. 535-36; Robinson 1932, p. 96; Liashchenko 1949, pp. 446-49). Nor, as tempting as the notion might be, was the growth in arrears a sign of peasant rebellion. The government insisted on the principle of "mutual responsibility," making the villagers collectively responsible for the dues of all households. Nor was the government hesitant to use force to "beat out" the tax, dispatching troops to seize and auction off the goods of entire villages. Non-payment indicated the sheer inability of peasants to pay: "An army of collectors heaved and strained at the task, but not all the king's horses and all the king's men could make the tale complete" (Robinson 1932, pp. 110-11).

In the midst of this growing crisis in peasant agriculture, the government adopted policies that preserved and even strengthened the solidarity and independence of the villages. In the emancipation reforms, the imperial government adopted the principle that the abolition of serfdom eliminated the basis for direct juridical control by the nobility, and therefore transferred local governmental authority to the village assemblies. At the same time, the drafters of the reform elected to preserve traditional forms of land tenure, most critically the repartitional commune (or *obshchina*), that insured all households equal rights to the use of the plowlands, commons, and forest collectively owned by the village. Whatever the advantages in administrative convenience and guarantees for the collection of redemption payments and taxes, the combination created a potent basis for collective action among the peasantry. The village assembly became an autonomous peasant government, collecting taxes and redemption money, selecting military recruits, handling local disputes, and coordinating repartition and land use. Moreover, collective land rights and the village cooperation required by the three-field system engendered a strong sense of solidarity among the villagers. Nor did the government act to weaken the arrangement; if anything, it strengthened the solidarity of the villages. During the "period of reaction" of the 1890s, further restrictions were placed on withdrawal from the commune. Peasants were required to secure permission from the local *zemski* chief, a nobleman appointed by the provincial governor to oversee the villages, and bans were placed on sales of allotment lands to nonpeasants and the use of allotment lands as collateral for private land

purchases. In addition, the government regularized the timing of repartition to a 12-year interval, stimulating a general increase in repartitions which, according to surveys of the Free Economic and Geographic Society, had declined to 35% of the villages in the 1880s but rose under official encouragement to 88% between 1897 and 1902 (cited in Volin 1970, p. 81). The general vigor of communal traditions within the villages in the 1890s is indicated by the fact that almost one-half of all peasant purchases of private lands and a majority of rentals were contracted through the village assembly rather than by individual peasants (Pavlovsky [1930] 1968, p. 109; Robinson 1932, pp. 123–24).

THE REBELLIONS OF 1905–7

The turn of the century marked a major increase in peasant unrest. While the Russian peasantry has been widely noted for its traditional rebelliousness (see Kolchin 1978), there had not been a major conflagration since the Pugachev rebellion in 1773–75. Uprisings had occurred, but they had been isolated *jacqueries*, generally focused on immediate grievances and, because confined to single estates, readily quelled by a handful of troops. The 1890s, however, witnessed a sharp increase in the tempo of these disorders and the incidence of violent attacks on the noble estates (Shapkarin 1959, pp. 601–48). In the spring of 1902, uprisings broke out simultaneously in over 160 villages in the Ukrainian provinces of Kharkov and Poltava. While the attacks were largely confined to grain seizures and the burning of outbuildings, the large-scale nature of the disorders led contemporaries to label them a “miniature revolution” (Robinson 1932, p. 138). This outbreak was to prove only a harbinger. In 1904 the imperial government became involved in a hopeless war with Japan. In response to heightened war requisitions and repeated military losses, over the winter and spring of 1905 the upper classes and the urban working class demanded political reforms. In the midst of this turmoil, the peasants renewed their attacks on the estates. This time the imperial troops were pinned down on the front and by efforts to contain the urban revolt, leaving the peasants a free hand. By the summer of 1905, the countryside was in a general tumult, the disorders having engulfed entire provinces and spread throughout most of the empire. The targets revealed quite clearly the nature of the peasant grievances. While clashes with state officials and police constituted 14.5% of the incidents, the overwhelming majority (75.4%) centered on the noble estates (Dubrovskii 1956, pp. 65–67). The specific actions varied widely—rent and wage strikes, the illicit cutting of wood, seizures of grain and stock, the burning of buildings, and open seizures of estate lands. The principal object, however, remained the same—control over the land. The disorders were generally confined to the inhabitants of single villages and emerged

with no more impetus than the rumor that similar uprisings had occurred elsewhere. Political agitation was relatively rare and the peasants expressed little interest in the occasional Socialist speaker who might appear in their midst. The peasant demands were quite direct. Rents should be abolished or at least reduced; "needless" taxes and service obligations should be eliminated; most important of all, noble lands that "rightfully" belonged to the peasantry should be returned at once (for a general account, see Prokopovich 1912, chaps. 3-5; Mavor [1914] 1965, 3:303-35; Shestakov 1926; Robinson 1932, pp. 115-27; Dubrovskii 1956).

The intensity of the rebellions was the greatest in the central black earth provinces, especially in those districts where the peasantry had retained their allotment holdings while becoming involved in smallholder tenancy and wage work. Of the 7,165 disorders officially reported, almost one-third took place in six central black earth provinces that contained slightly less than one-sixth of the peasant households (Dubrovskii 1956, p. 80). This was also the center of the most militant actions. In the outer provinces, the disorders were primarily limited to strikes for higher wages or lower rents and were less likely to entail violence or significant property damage. In the central provinces, however, the principal object of struggle was control over the land. While the uprisings might begin with minor forays to test the nobleman's strength, the clashes generally culminated with the entire village marching *en masse* on the estate to demand that the lands be transferred to the control of the village. Whether the demands were couched in terms of claims to the cutoffs that had been seized by the nobleman's ancestors or in terms of the "labor principle" that the land belonged to those who tilled it, the object was the same—immediate control over the land. Having carted off the grain stores, work animals, and farm implements, the peasants "put the red cock to the roof" of the nobleman's château to insure that the inhabitants would never return. In some villages, the peasants carried out a "black repartition," redistributing the newly seized lands alongside the village allotment lands.

Participation in the revolts varied among the different segments of the villages. According to the detailed reports compiled by the Imperial Free Economic Society, the land seizures were typically carried out by entire villages, often organized by the village assembly. The most consistent support came from the "middle" peasants who still held allotments but had become involved in cash tenancy and wage work. The younger peasants, who were more likely to hold inadequate allotments and to have been involved in "go-away" work, were also likely to support the rebellions. Rich peasants were more likely to remain aloof, especially if they held private lands or hired other peasants to work their lands. The rebellions received weak support from the poorer households, especially the landless

peasants who were dependent on the owners of the estates for a livelihood (Robinson 1932, pp. 155–57; Perrie 1972, pp. 127–44).

After the rebellions reached a peak in the summer of 1906, the imperial government managed to regroup its military forces and began systematically suppressing the uprisings. The urban revolt that had culminated in the St. Petersburg soviet was crushed, its leaders in flight or in prison. The revolutionary forces were in disarray, the liberals having decided to accept the limited constitutional reforms offered by the tsar while the Socialists campaigned for a popular boycott of the new Duma. Relieved of the pressure of the urban revolt and reinvigorated by the infusion of massive loans contracted on the French *bourse*, the government dispatched troops to the countryside to restore order. Though a third round of rebellions occurred in the spring of 1907, the risings were readily suppressed.

The general course of socioeconomic changes and the character of the rebellions, then, do not clearly rule out either the structural or the historical theory. Smallholder tenancy was increasing rapidly. At the same time, the economic security of the peasantry was being threatened by the expansion of the commercial market, increasing exactions by landowners and the state, and rapid population growth. In this context, traditional village institutions provided a significant measure of peasant autonomy. Were these features connected in a systematic way to the incidence of the rebellions?

METHOD, DATA, AND VARIABLES

The various agricultural censuses conducted by the Russian government after 1887 and the systematic data collected by Russian scholars tracing changes in village institutions and the peasant economy provide a solid basis for evaluating the competing theories of peasant rebellions. The principal method will be regression analysis. Throughout, the unit of analysis will be the province, the major administrative unit of the imperial government. While the provinces were quite large and varied widely in population, they were relatively homogeneous internally with regard to land tenure, crops, and village institutions. Therefore the problem of spurious correlation appears to be minor.

The structural theory argued that the shift from a manorial economy to smallholder tenancy simultaneously increased the intensity of peasant grievances and the solidarity of the villagers. The major issue, then, is the extent to which the peasantry had become involved in smallholder tenancy, renting lands from the nobility and urban landowners either on shares or cash equivalents. Unfortunately, provincial statistics on the percentage of peasant households leasing land do not exist. The best available information derives from the report of the Imperial Commission on the Needs of the

Peasantry (1903) that records the acreage of private lands leased by peasants in 1900–1901 (Prokopovich 1924, pp. 136–37). Though the nature of peasant renting varied from region to region, subsistence or smallholder renting prevailing in the central districts and entrepreneurial renting in the east and south, the overwhelming majority of the rented plots were small units of 8–10 *desiatinas* (Volin 1970, pp. 73–74). The best index of the proportion of households involved in smallholder tenancy, then, is the ratio of the amount of leased land to the number of peasant households (tenancy).

The historical theory is empirically more complex, specifying multiple sources of grievances and bases for mobilization. Peasant grievances centered on threats to an economic subsistence posed by the expansion of the market, the increased exactions of landowners and the state, and rapid population growth. The potential for mobilization depended on two features: (1) the strength of village solidarity based on the predominance of “middle” peasants and strong communal traditions rooted in collective land rights and village cooperation; and (2) the independence of the peasants engendered by inalienable land rights and autonomous village government. Several of these factors, especially increased tax burdens and autonomous village government, were relatively uniform throughout the empire. While they could have shaped the changing potential for rebellion in the empire as a whole, they probably did not affect regional variations in the rebellions.

The expansion of the market economy affected peasants in two ways, fostering increased production for the market and increased entry into the market to secure access to land. Because wheat was the chief cash crop for peasants, the proportion of peasant production oriented toward the market (grain market) can be captured by the proportion of cultivated land in each province that was devoted to wheat production in 1901–5 (Oganovskii 1923, pp. 78–91). (Changes in the amount of wheat production would capture more accurately the meaning of the theory, focused as it is upon increased market participation, but production figures for earlier years are unavailable.) Peasants entered the land market by both renting and private purchases. Because the extent of tenancy forms the heart of the structural theory, the most useful index for the historical theory (land market) is the increase in the amount of private lands owned by peasants between 1887 and 1905 (Oganovskii 1923, pp. 54–58).

While precise estimates of the economic surplus extracted by the landowners are unavailable, figures on one of its key sources—increased leverage due to population growth—can be assessed. Because the peasants were effectively tied to the land, the rapid increase in the village population simultaneously eroded the subsistence guarantees provided by allotment holdings and strengthened the capacity of the landowners to extract an increased economic surplus through higher rents and lowered wages. The

economic "squeeze" should therefore have been the most intense in provinces in which the rural population density was the highest (population density), and in which the average allotment size (allotment size) was the smallest (Oganovskii 1923, pp. 18-21, 54-58, 78-91).

If peasant solidarity rested primarily on the predominance of the "middle" peasantry and the strength of communal institutions, the incidence of rebellions should have been linked to the proportion of peasants holding medium-sized allotments and to the prevalence of collective land rights. The 1905 land census provides information on the size of allotment holdings, reporting the proportion of households with small (1-5 *desiatinas*), medium (5-10 *desiatinas*), and large (10+ *desiatinas*) holdings (Oganovskii 1923, pp. 66-73). According to the "middle" peasant thesis, the greater the proportion of households with medium-sized allotments, the greater should have been the solidarity of the villages (middle peasantry). While the three-field system prevailed throughout most of the empire, giving rise to a basic minimum of village cooperation, the incidence of collective land rights varied widely. Peasants held allotments under two arrangements—communal and hereditary tenure. Under communal tenure, allotment lands were held collectively by the village. Plowlands were periodically repartitioned by the village assembly according to the size of the households. Though cultivation remained in the hands of individual households, periodic repartition and collective obligations for redemption payments, taxes, and state service should have engendered significant village solidarity (Watters 1969). Under hereditary tenure, allotment lands were permanently assigned to individual households. While the three-field system and the village assembly undoubtedly knitted the village community together, the degree of village solidarity should have been significantly greater under communal tenure (communal tenure). The measure used here is the percentage of villages in each province holding land under communal tenure according to the land census of 1905 (Atkinson 1976, pp. 785-86).

The rebellions had hardly subsided before the imperial government began collating provincial reports on the intensity of the uprisings. The evidence is the weakest on the militancy of the actions. Whether peasants merely burned crops and struck for lower rents or seized the estates and murdered the inhabitants is unclear. While militancy appears to have been the greatest in the same regions that experienced the greatest incidence of disorders (Dubrovskii 1956, pp. 65, 67), precise figures on a provincial level are lacking. What does exist is a reasonably accurate record of the number of disorders in each province (Prokopovich 1912, pp. 64, 94, 108) and the proportion of the villages in each province that became involved (Liashchenko 1949, p. 743).

While both measures of the incidence of the rebellions will be used, the latter is probably the more valid measure. Provincial officials collecting

evidence were probably more likely to keep an accurate record of the number of unruly villages than of the precise number of disorderly incidents. Nor is it clear that a village that engaged in a series of rebellious actions before finally seizing the lands was more rebellious than the village that "put the red cock to the roof" at the outset. Moreover, counting the proportion of villages involved rather than the number of disorders meshes more closely with an important feature of the rebellions—that it was entire villages, not single households, that carried out the actions (cf. Kolchin 1978; Keep 1976, pp. 200–217).

The measures used are the number of disorderly incidents reported for each province divided by the size of the peasant population (incidents) and the percentage of the villages (or more accurately *uezds*—most *uezds* contained only one physical village but some contained two or more) in each province that experienced a rebellious event (village involvement). A scale of 0–3 was used for each measure. That is, the number of incidents was recorded as: 0 = less than 1 per 1,000 peasant population; 1 = 1–2 per 1,000 peasant population; 2 = 2–3 per 1,000 peasant population; and 3 = 3 or more per 1,000 peasant population; and the involvement of the villages was recorded as: 0 = less than 25% of villages; 1 = 25%–49% of villages; 2 = 50%–74% of villages; and 3 = 75%–100% of villages. Not only would it be difficult to surpass the efforts of the analysts who have resorted to this scaling, but also, in the light of the reliability of the police reports on which the data are based, a simplified scale based on broad categories is probably more valid than a misleadingly precise enumeration.

RESULTS OF THE ANALYSIS

While the extent of tenancy in Russian agriculture clearly increased during the late 19th century, this analysis corroborates Chirot and Ragin's (1975) finding that tenancy has little significance in shaping the incidence of peasant uprisings (tables 2 and 3). Not only was the amount of explained variance extremely small ($r^2 = .02$ and $.01$) but, once controls are introduced for the effects of the market, the relatively weak relationship between tenancy and the incidence of rebellions is eliminated (table 3). In contrast, most of the factors identified by the historical formulations—the intrusion of the market, the shrinking size of allotments, population growth, and the existence of middle peasants—appear to have contributed significantly to the rebellions. The historical factors combined account for almost one half of the variation in the distribution of rebellions ($R^2 = .46$ and $.49$).

Among the historical factors, the most important are those linked to the land hunger of the peasantry and the middle peasantry's capacity for mobilization. Not only had the nobles cut off a significant portion of peasant lands and imposed heavy redemption obligations, but also rapid population

TABLE 2
SOURCES OF PEASANT REBELLION (Single-Order r 's)

	Incidents	Village Involvement	Tenancy	Grain Market	Land Market	Population Density	Allotment Size	Middle Peasantry	Communal Tenure
Incidents.....	1.00								
Village involvement.....	.36	1.00							
Tenancy.....	.13	-.11	1.00						
Grain market.....	.26	.26	.19	1.00					
Land market.....	-.05	.17	-.18	.13	1.00				
Population density.....	.46	.57	-.28	-.05	.18	1.00			
Allotment size.....	-.36	-.32	.24	.15	-.41	-.65	1.00		
Middle peasantry.....	.40	.27	-.14	.06	.44	.28	-.72	1.00	
Communal tenure.....	-.08	.07	.15	.06	-.27	-.54	.15	.37	1.00

TABLE 3
INDEPENDENCE OF THE SOURCES OF REBELLION
(Partial r 's)

Variables Controlled For	Incidents	Village Involvement
Tenancy:		
Grain market.....	-.02	-.21
Land market.....	.07	-.10
Population density.....	.15	.04
Allotment size.....	.21	.01
Middle peasantry.....	.09	-.04
Communal tenure.....	.11	-.12
Grain market:		
Tenancy.....	.24	.35
Land market.....	.26	.30
Population density.....	.29	.41
Allotment size.....	.31	.38
Middle peasantry.....	.20	.29
Communal tenure.....	.26	.32
Land market:		
Tenancy.....	-.02	.17
Grain market.....	-.08	.15
Population density.....	-.14	.08
Allotment size.....	-.24	.04
Middle peasantry.....	-.26	.07
Communal tenure.....	-.07	.17
Population density:		
Tenancy.....	.48	.57
Grain market.....	.45	.59
Land market.....	.48	.62
Allotment size.....	.29	.48
Middle Peasantry.....	.41	.53
Communal tenure.....	.47	.62
Allotment size:		
Tenancy.....	-.45	-.34
Grain market.....	-.45	-.42
Land market.....	-.47	-.32
Population density.....	-.18	.04
Middle peasantry.....	-.20	-.22
Communal tenure.....	-.39	-.35
Middle peasantry:		
Tenancy.....	.44	.27
Grain market.....	.41	.28
Land market.....	.49	.25
Population density.....	.38	.16
Allotment size.....	.25	.08
Communal tenure.....	.47	.32
Communal tenure:		
Tenancy.....	-.16	-.09
Grain market.....	-.15	-.12
Land market.....	-.15	-.06
Population density.....	.15	.31
Allotment size.....	.02	.04
Middle peasantry.....	-.23	-.15

growth had undercut the security afforded by allotment lands and strengthened the hand of the landowners. These two factors alone—the shrinking size of allotments and the increasing density of the rural population—accounted for over one-quarter of the variation in the distribution of the uprisings ($R^2 = .24$ and $.31$). While increasing peasant participation in the grain market appears to have heightened peasant insecurity ($r = .26$ and $.26$), the factors pointed to by the exploitation theory had greater relevance, as shown in table 2. In fact, the market factors received only mixed support, the expansion of the private land market operating to reduce the number of incidents ($-.05$) and only marginally increasing the likelihood of village involvement ($.17$). Overall, by compensating for the small size of allotments, private land purchases appear to have reduced the land hunger of the peasantry instead of increasing their economic insecurity. That increased private holdings were inversely related to tenancy ($-.18$) and the size of allotments ($-.41$) and positively linked to the presence of middle peasants ($.44$) supports this interpretation. Nor does the expansion of the private land market hold up as an independent factor once controls are introduced for the other variables shaping the rebellions (table 3). The expansion of the grain market, however, appears to have had an independent significance apart from the other historical changes, its significance remaining intact or even increasing with the introduction of controls (table 3). Strikingly, the same holds for each of the other factors, except for the closely interrelated changes in population density and the size of allotments. The main dynamic giving rise to peasant grievances appears to have been increasing peasant land hunger attributable to the heritage of the emancipation land settlement, the increase in rural population, and the increasing exactions from above. Increased participation in the commercial grain market, in turn, exacerbated peasant insecurity by increasing the margin necessary for a secure subsistence.

While the existence of a strong middle peasantry was clearly related to the uprisings ($r = .40$ and $.27$), this analysis leaves unclear to what extent this was due to their stronger social ties to the villages or their relative economic independence from the large landowners. In view of the persistence of the relationship despite the introduction of controls for the intrusion of the grain market, it appears that these factors were more important than a cultural factor, namely, the middle peasantry's economic conservatism (cf. Wolf 1969, pp. 291–92).

The most striking finding in the analysis is the apparent irrelevance of the land commune in mobilizing the rebellions ($r = -.08$ and $.07$). Both contemporary observers and subsequent analysts of the rebellions (Robinson 1932, p. 153; Owen [1937] 1967, p. 8; Moore 1966, p. 476; Perrie 1972, p. 138) have consistently pointed to the commune as a major source of native peasant "radicalism." In fact, the imperial government was suffi-

ciently convinced that immediately following the rebellions it embarked upon the Stolypin reforms, an attempt to dismantle the commune and transfer the peasants to private plots, preferably isolated homesteads or *khutory*. While this might be the result of problems with the data (especially the limits of provincial data in assessing a village-level phenomenon), it seems more likely that communal tenure had relatively weak significance because of the prevalence of other sources of solidarity that are unmeasured here (especially autonomous village government, the three-field system, and village cooperation) and the fact that communal tenure operated to forestall the economic squeeze on the peasantry by periodically reallocating increasingly scarce lands (cf. Scott 1976, pp. 202-3). In other words, communal tenure served to keep the middle peasantry intact ($r = .37$), redistributing scarcity and thereby softening the impact of increasing insecurity.

CONCLUSIONS

The structural sources of peasant rebellions lay in the economic insecurity of the peasantry and their possession of an independent basis for collective action in the villages. While the historical analysis furnished provisional support for both approaches, the regression analysis ruled out the structural argument concerning tenancy and corroborated the basic propositions of the historical theory. The inadequate size of allotments, the rapid growth of the rural population, and the expansion of the grain market directly threatened the security of the peasant's claims to an economic subsistence. As the historical analysis of the changes under the *ancien régime* indicated, these patterns were products of increasing exactions by the landowners and the state upon a relatively stable base of peasant production. At the same time, these changes left intact traditional village institutions centered on the middle peasantry and strengthened the independence of village government, thereby furnishing a basis for peasant mobilization against these threats. Presumably studies that have found a close link between tenancy and the incidence of peasant rebelliousness (Mitchell 1969; Zagoria 1971) have done so because of underlying links between tenancy and peasant insecurity.

The precise institutional arrangements and historical sequences that have given rise to the 20th-century peasant wars are no doubt diverse enough to make generalizations from one case hazardous. In fact, the uneven results for the market thesis suggest that any attempt to link modern peasant rebellions consistently to a single factor such as the global expansion of the capitalist world economy (see Wolf 1969) is doomed to failure. A more viable approach would recognize the distinctiveness of the institutional structures and historical changes in particular societies that have simultaneously undermined peasant security while providing bases for

mobilization. Certainly the changes undermining peasant security and the institutions furnishing solidarity need not look precisely like those in Russia. The intrusion of the market might play a key role in this dynamic, depending on such other factors as the success of the landowners in reorganizing their estates as commercial farms, the adequacy of peasant holdings, and the political and social relations between landowners and the peasantry. The key factors that should show up in other cases are economic changes that endanger the claims of the peasantry to an economic subsistence and institutions, traditional or new, that provide an organizational base for collective action.

In the Russian case, the major changes were rooted in long-standing institutional features of the Russian state and the increasing politico-military competition confronted by the imperial government in the late 19th century. During the early formation of the Russian state, the landowning nobility was subordinated to the central government. Landownership was made virtually synonymous with lifetime military service in the imperial government, giving rise to a strong tradition among the nobility of pursuing careers within the army or the state bureaucracy and leaving their estates largely in the hands of the peasants. While this arrangement had made the Russian state into a major European power by the late 18th century, it soon proved to be an economic hindrance to the continued pre-eminence of the empire. By the mid-19th century, the breakthrough to capitalist development had strengthened the military position of the Western European states. In order to strengthen its politico-military position, the imperial government began experimenting with internal reforms. In the emancipation, the government increased its capacity to conscript peasants into the military but, by keeping the peasantry tied to the land and leaving the economic privileges of the nobility largely intact, hindered agricultural development. The majority of the nobles adhered to their servitor traditions, leasing out their estates in small plots and pursuing careers as state officials rather than embarking on large-scale commercial farming. Because the peasants were immobile and left with inadequate allotments, they were forced back into dependency on their former masters. With the rapid increase in rural population and the general rationalization of the system of legal enforcement, the hand of the landowners was gradually strengthened. The next round of military-centered reforms came in the 1890s when the imperial government mounted a hothouse industrialization program, primarily at the expense of the peasantry. State monopolies on consumer goods and high protective tariffs on imported industrial goods taxed peasant incomes; land taxes levied on the peasantry were increased to support extensive foreign loans and state railway construction; an increasing portion of the grain harvest was squeezed out of peasant hands

into the international grain market to generate a favorable balance of payments that facilitated foreign investments in selected heavy industries. Given the parcellized land system and the obstacles to improvements in productivity, the weight of these demands ultimately rested primarily on the backs of the peasantry. Finding their economic margin trimmed to the limit and possessing a strengthened basis for collective action, the peasantry rebelled.

In 1917 a second round of rebellions emerged that paralleled in most respects those of 1905 (Keep 1976, pp. 186–216). In both cases, the rebellions occurred within the context of a generalized politico-coercive crisis created by major war losses that had seriously undermined the capacity of the imperial government to contain dissidence by repression. Does this mean, as Paige (1975, p. 42) and Skocpol (1979, pp. 133–36) have argued, that the development of such a coercive crisis constitutes a necessary precondition for the conversion of isolated *jacqueries* into large-scale rebellions? Clearly the precise timing of the rebellions depended on the collapse of the government's coercive capacity. Moreover, it seems reasonable to assume that the uprisings of 1902 that preceded the conflagration of 1905–7 would have turned into a generalized rebellion had the imperial government faltered in its repressive efforts. The structural potential for peasant rebellion, however, was not a constant that existed to the same degree in, say, 1870. As the analysis has shown, changes in economic organization and village institutions played a significant role in determining the regional incidence of the disorders. At least in 1905, these regional variations were clearly not due to differences in the state's coercive capacity. Likewise, the heightened pace of isolated uprisings running from the 1890s through 1905 suggests that the structural potential for rebellion was gradually increasing. The importance of the Paige/Skocpol thesis on the coercive crisis is that it highlights the special organizational problems of peasant uprisings. Lacking large-scale organizational bases for collective action, peasant uprisings tend to be confined to single villages and therefore readily repressed. Unless a coercive crisis undermines the capacity of the state to contain these uprisings, rebellions remain isolated *jacqueries*. If, however, a more extensive organizational base is provided by the mobilization efforts of a party outside the villages, such as urban organizers or a revolutionary party, then the fragmentation of the peasantry may be overcome, giving rise to large-scale rebellions without a coercive crisis (cf. Wolf 1969, p. 290; Migdal 1974, p. 232). The political role of the peasantry therefore depends on the actions of other groups. Peasant rebellion might provide the "dynamite," but other groups must provide the organizational basis before rebellions become sufficiently large in scale to usher in major political and economic changes.

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Christian Intolerance of Homosexuality¹

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From late antiquity to the Middle Ages there was historical variability in Christian responses to homosexuality. This paper traces Christian intolerance of homosexuality to the ascetic movements that arose from the social crises of the ancient Mediterranean world and to the Gregorian reforms of the medieval Church.

Historical and anthropological research has documented wide variability in the social acceptance of homosexual activity. In some times and places, some forms of homosexual interaction have been fully institutionalized, sometimes serving religious and educational functions. Yet homosexuality² has also met with hostile responses ranging from mild disapprobation or ridicule to imprisonment and execution (Matignon 1899; Carpenter 1919; Ford and Beach 1951; Karlen 1971; Dover 1974, 1978; Bullough 1976; Katz 1976; Trumbach 1977; Ungaretti 1978; Boswell 1980; Bremner 1980).

Despite the attention the labeling theory literature of the past two decades has given to the creation of deviant categories, explanations of this variability are few. Textbooks either ignore the issue or deal with it cursorily with reference to the Judeo-Christian tradition. But this approach leaves many questions unanswered. Why did Judaism and Christianity prohibit homosexual activity when other religions did not? If the early Christian church abandoned some Jewish practices (such as dietary restrictions, circumcision, and observance of Saturday as the Sabbath), why did it preserve others? If, in the course of centuries, Christians modified or aban-

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² Because evidence about lesbianism is so limited, much of what we have to say concerns male homosexuality. The context will make clear whether references to homosexuality refer to men alone or to men and women.

done some early doctrines, such as the prohibition of usury and, for Protestants, priestly celibacy, why not all? If the condemnation of homosexuality in other religions had little if any impact on popular attitudes,⁸ why did it have such a powerful impact on Jews and Christians? Moreover, the assumption of some authors (e.g., Crompton 1978) that the Christian West has been marked by relentless persecution of homosexuals over a period of two millennia has not been demonstrated. Indeed, several recent studies suggest that the treatment of homosexuality in Christian Europe over the last two millennia has been extremely variable (Roby 1977; Goodich 1979; Boswell 1980).

Boswell (1980) has linked this variability to urbanization, arguing that exposure to the cultural diversity of cities makes people more tolerant. Intolerance of homosexuality is thus assumed to be more prevalent in culturally homogeneous rural areas. This reasoning leads Boswell to attribute the growth of intolerance toward homosexuality in the late Roman Empire to the deurbanization that accompanied economic decline and the barbarian invasions.

There are reasons for questioning this logic. As Boswell himself notes, the period of repression that began in 13th-century Europe was not accompanied by a reduction in the number or size of towns. If anything, Europe was becoming more urban. The little surviving evidence about the barbarian Germans does not indicate that they were particularly hostile to homosexuality (Bremmer 1980). The anthropological literature cited above documents the existence of many nonurbanized peoples who were not generally hostile to homosexual expression. For example, in the mountains of early 20th-century Albania—a region far from urbanized—male homosexual marriages were celebrated in churches (Näcke 1965). Homosexual relationships between men and boys were or are virtually universal among the Papuans and the Sambia of New Guinea (Williams 1969; Herdt 1981). While evidence from Europe and the United States over the past few centuries does suggest that cities facilitate the emergence of homosexual subcultures, the very visibility of these subcultures may elicit hostile attacks.

For these reasons, the exploration of alternative hypotheses seems warranted. Here we will examine the contributions made by the economic and political transformations of the late Hellenistic and Roman empires and

⁸ For example, the Koran derogates homosexual acts and, though it does not clearly specify a penalty, seems to include them among other punishable sexual derelictions (for a discussion of the relevant passages see Bullough [1976], pp. 221–22), yet open celebrations of marriages between men have been reported from parts of the Maghreb, and quite favorable attitudes toward homosexuality have prevailed in some Moslem societies (see, e.g., Boswell [1980], pp. 194–96, and Hodgson [1974], pp. 145–46). No efforts were made to suppress the homosexual relationships that developed frequently in Tibetan monasteries despite the vow of chastity required of all Buddhist monks (Prince Peter 1963).

the church reforms of the Middle Ages to the crystallization of a distinctly Christian intolerance of homosexuality. We will argue that early Christian views of sexuality were formed in the context of a broad trend toward asceticism in the Hellenistic and late Roman empires and of the competition between what was eventually recognized as the orthodox Christian church and other religious cults, including the Gnostics. Although the early Church made attempts to forbid and control homosexuality, harsh repression, carried out consistently over an extended period, began only in the high Middle Ages. We link this development with the Gregorian reforms in the Church, the rise of a centralized monarchy, and the growth of class conflict in medieval city-states.

HOMOSEXUALITY IN THE ANCIENT WORLD

The ancient Greeks attached no great social significance to the sex of sexual partners. Where arguments about the relative merits of male and female lovers are recorded, the two are accorded equal status. No conception of a homosexual person existed; sexual object choices were not thought to be, and generally were not, mutually exclusive. This is implicit in the comments made by the Greek historian Alexis about Polycrates, the wealthy ruler of 6th-century Samos. Alexis expresses his astonishment that even though Polycrates had imported many expensive goods, 'the tyrant is not mentioned as having sent for women or boys from anywhere, despite his passion for liaisons with males' (quoted in Padgug [1979]).

Male homosexuality among the ancient Greeks has been regarded commonly as having arisen as an expression of the "comradeship of arms" among noble warriors in the Heroic Age or as part of a primitive religious initiation ceremony for young men. In the classical period it usually involved temporary relationships between youths and older men. These relationships played an important role in the young men's education, particularly that of the aristocrats, and were utilized to strengthen the zeal of soldiers; indeed, the Theban army composed of homosexual lovers was reputed to be undefeatable (Licht 1963; Flacelliere 1962; Ungaretti 1978; Dover 1974, pp. 213-15; 1978, p. 16; Bullough 1976, pp. 93-126; Bremner 1980).

Age-asymmetric sexual relationships among males are not uncommon in highly patriarchal societies.⁴ In these societies, all sexual relationships are conceptualized in terms of heterosexual relationships involving superordination and subordination. Just as men dominate women in heterosexual rela-

⁴ We use the term "patriarchy" to refer to the personal domination of a male head of household over the other members of the household (Weber 1968, pp. 1006-7), not as a synonym for all forms of sexual domination of women by men, which is an imprecise usage found in much recent literature on gender.

tionships, so one man dominates another man or a boy in a homosexual relationship. This subordination is considered demeaning to the submissive partner, who is regarded as a "woman" in the relationship, but not to the controlling partner, whose superior masculine status is confirmed by his domination of another man⁵ (Vangaard 1972; Ungaretti 1978; Dover 1978, pp. 76-109; Bremmer 1980).

How little of Greek thinking about sexual matters was governed by the sexes of the partners and how much by their relative social positions can be seen in *The Interpretation of Dreams* of Artemidorus Daldianus, a Greek of the 2d century A.D.⁶ The section on sexual dreams indicates that

having sexual intercourse with one's servant, whether male or female, is good; for slaves are possessions of the dreamer, so that they signify, quite naturally, that the dreamer will derive pleasure from his possessions. . . . If a man is possessed by a richer, older man, it is good. For it is usual to receive things from such people. But to be possessed by someone who is either younger than oneself or destitute is unlucky. For it is usual to give things to such people. The same also holds true if the possessor is older but a beggar. . . . Possessing a brother, whether he is older or younger, is auspicious for the dreamer. For he will be on top of his brother and disdainful of him. And whoever possesses his friend will become his enemy, since he will have injured his friend without provocation. [1975, pp. 59-60]

A juvenile male can be dominated by older men in a patriarchal society without incurring a stigma because the subordination of the young is a "natural" (and, for any individual, temporary) feature of a patriarchal social structure. The ideal homosexual relationship in ancient Greece conformed to this pattern, and most actual relationships appear to have done so as well, though exceptions to this generalization were not unknown.⁷

Some scholars have maintained that homosexuality never found the same acceptance among the Romans as among the Greeks (Bullough 1976, pp. 137-38); however, Boswell (1980, pp. 61-87) has amassed considerable evidence challenging this conclusion. Although expressions of distaste for homosexuality can be found in Roman literature, negative attitudes seem

⁵ This pattern of responses has been documented for contemporary Mexico by Carrier (1977). The distinction between "masculine" and "feminine" homosexual roles seems to be limited to societies where gender distinctions are generally important as social categories. Some evidence suggests that homosexual relations and male transvestism are more acceptable in societies where the sexual division of labor is low, male dominance is weak or nonexistent, and personality distinctions among the sexes are neither pronounced nor linked to sexually based role categories (Hill 1935; Munroe, Whiting, and Hally 1969; Levy 1973; Kluckhohn and Leighton 1976). This comes as no surprise: we should expect the sex of one's sexual partner to be relatively unimportant in such societies.

⁶ We are grateful to Michel Foucault for calling this text to our attention.

⁷ Thus Dover (1978, p. 16) states, "The reciprocal desire of partners belonging to the same age-category is virtually unknown in Greek homosexuality," but some of the vase paintings he reproduces in his plates do show same-age homosexual encounters.

to have centered on effeminacy, coercion, the seduction of minors, and the participation of citizens, but not foreigners or slaves, in prostitution. There was probably no law against homosexuality per se until fairly late in the Empire.⁸

Apparently the Romans did share the role stereotypes of the Greeks. According to Boswell,

a very strong bias appears to have existed against passive sexual behavior on the part of an adult male citizen. Noncitizen adults (e.g., foreigners, slaves) could engage in such behavior without loss of status, as could Roman youths, provided the relationship was voluntary and nonmercenary. . . . But if an adult citizen openly indulged in such behavior, he was viewed with scorn. Apart from general questions of gender expectations and sexual differentiation, the major cause of this prejudice appears to have been a popular association of sexual passivity with political impotence. Those who most commonly played the passive role in intercourse were boys, women, and slaves—all persons excluded from the power structure. Often they did so under duress, economic or physical; and the idea that a Roman citizen should be exploited in this way evoked a particular horror among Romans who prided themselves on their control of the world around them. [1980, pp. 74–75]

Similar patterns of role differentiation in the acceptability of homosexuality prevailed among the Germanic tribes (Bremmer 1980), in feudal Japan (Karlen 1971, p. 231), in the Moslem Middle East (Coon 1931, pp. 110–11; Patai 1973; Westermarck 1926; Hodgson 1974, pp. 145–46), among the Azande in Africa (Evans-Pritchard 1970), and among the Papuans and Sambia of New Guinea (Williams 1969; Herdt 1981). The accounts of the conflict between Horus and Seth in Egyptian mythology (Bullough 1976, pp. 64–66) and the middle Assyrian laws against the spreading of false rumors concerning passive homosexuality (Pritchard 1955, p. 181) suggest that similar attitudes were held in ancient Egypt and Mesopotamia.

Although Livy reports that male homosexual intercourse was part of the Bacchic rites in the early Roman republic, little is known about these ceremonies. However, in the ancient Near East, homosexual prostitution was a feature of the established religious cults (2 Kings 23:7; Driver 1895, p. 264; Selbie 1902, p. 559; Becker 1964; Bullough 1976, pp. 51–58). Though suppressed periodically by those loyal to the worship of Jehovah, cult prostitution—heterosexual and homosexual—was never eliminated permanently in the biblical period.

The early Mesopotamian law codes (Sumerian, Babylonian) make no mention of homosexuality; the Hittite code prohibits only father-son incest

⁸ The history of this legislation remains controversial; in particular, the interpretation given to the statutes of 342 and 390 A.D. is currently under debate; compare Boswell (1980, pp. 123–24) with Lauritsen (1981).

(Friedrich 1971, pp. 83, 113–14); and the middle Assyrian code forbids only the homosexual rape of a neighbor.⁹

The Spread of Asceticism

This broad acceptance of homosexuality in the ancient Mediterranean world ended in late antiquity with the spread of an asceticism hostile to all forms of sexual pleasure, including homosexuality. This asceticism was associated typically with philosophies or religions based on dualistic oppositions of good and evil, spirit and flesh, male and female.

Ascetic attitudes toward sexuality contrast radically with those usually found in primitive agricultural societies. The religions in these societies often feature an opposition between male and female principles which may well reflect a social antagonism between the sexes stemming from a sexual division of labor but does not assert the superiority or triumph of either principle. In the context of plant and animal reproduction this would be ludicrous. What is found instead is a unity in opposition, often symbolized by androgynous, hermaphroditic, or transvestite gods (Eliade 1964; Campbell 1970; Harding 1976). Sexual pleasure is valued positively; the phallic symbol itself may be worshiped (Knight 1786; Stone 1927; Vangaard 1972) and male homosexual intercourse regarded as a means of incorporating into the young the strength and vigor of the inserter and of encouraging a young person's growth (Westermarck 1926; Schieffelin 1976). In antiquity, even where the relationships between men and women were more patriarchal than equal and the economy was not exclusively agricultural, sexual pleasure was valued positively.

The processes that gave rise to dualistic world views and antisexual ideologies differed in detail and timing in different parts of the Mediterranean world. Before turning to these details, we note four developments that occurred throughout the entire region.

1. The growth of long-distance trade and imperial expansion (the Hellenistic kingdoms, the Roman Empire) brought about increased contact among adherents of different religions. This led to syncretism but also fostered skepticism about traditional pantheons and their associated rites, including fertility cults whose worship included homosexuality. It also favored the development of transnational monotheistic religions.

2. As large cities grew and became important administrative and reli-

⁹ The standard translations (Driver and Miles 1935, p. 391; Cardascia 1969, pp. 133–34) of the relevant passages are misleading in suggesting that the prohibition extended to all homosexual acts. The verb used, *na-ku*, carries connotations of force and coercion that these translators overlook. The penalty specified (homosexual rape followed by castration) would make no sense if the prohibition applied to all forms of homosexuality. We are grateful to Robert Stieglitz for clarifying the translation.

gious centers, the cultural significance of the agricultural and fertility themes expressed by the polytheistic religions was weakened.

3. The larger scale of politics in the kingdoms and empires reduced popular participation in politics, giving rise to political estrangement, passivity, helplessness, and withdrawal.

4. Catastrophic wars and conquests so shook the national existence of the various Mediterranean societies that all sense of confidence and certainty about the world was shattered, leading many to withdraw from mundane concerns to lives of contemplation or spirituality.

For the Hebrews, dualistic and ascetic tendencies became prominent during the Second Commonwealth, following the return from the Babylonian exile. At this time the worshipers of Jehovah formed an urbanized community that had just gone through the shock of conquest and captivity; they were emotionally ready for asceticism. Epstein indicates that it was in this period that nazaretic vows of celibacy became popular and that rigorous regulation of sexual expression was introduced, a drastic departure from earlier custom (1967, pp. 6-11). Bullough (1973, p. 75) indicates that Hebrew literature written between the Babylonian exile and the Roman conquest viewed sexual desire as man's greatest weakness (in the Talmudic period, when Jews enjoyed greater security, there was a more positive view of sex).

The only general prohibitions against male homosexuality in Hebrew scripture appear in Lev. 18:22 and 20:13 (in addition, Deut. 23:18 prohibits male and female cult prostitution). Scholars have dated the Holiness Code in Leviticus, of which this passage is a part, as postexilic, and concluded that it was probably written in Judea, not in Babylonia¹⁰ (Eissfeldt 1965, pp. 238-39; Talmon 1970, p. 159; Epstein 1967, p. 136).

Although the shock of the Roman conquest of Jerusalem and the destruction of the Temple in A.D. 70 (Grant 1966, p. 27) must have reinforced an ascetic outlook, it was not unknown before this. Many Essene groups of the time practiced celibacy. The recently published text of the Temple Scroll of the Qumran community, probably composed in the 2d century B.C., calls for a reconstituted Jerusalem in which all residents were to remain sexually continent (Milgrom 1978).

¹⁰ The dating of the Holiness Code in Leviticus is no longer as certain as it once was. Early in this century, following the discovery of the Hammurabi Code, many textual parallels led scholars to suppose that the Leviticus laws derived from Babylonian sources. Now we know that similar codes were in effect throughout the Near East. This makes the dating uncertain and controversial. It is quite likely that the Leviticus prohibition of homosexuality had earlier sources, the most obvious being the middle Assyrian law code, inscribed on tablets dating from the 12th century. However, the Leviticus prohibition is broader in scope and specifies a different penalty. Our analysis favors a late date for the present form of the Leviticus prohibition. We are grateful to Robert Stieglitz for a discussion of the problems of establishing the dates of the Leviticus laws.

In Greece, as production was increasingly carried out by slaves, manual labor and, by extension, the material world became discredited, providing the social basis for a philosophical dualism asserting a radical dichotomy between mind and body, good and evil.¹¹ A more specifically ascetic orientation can be traced to the aftermath of the Peloponnesian War, the formation of the Hellenistic kingdoms (which were at war almost continuously), and their eventual conquest by Rome.

Pre-Christian Greek philosophers responded to the uncertainty of the times by attempting to renounce worldly cares and pursuits. Thus Epicurean communities sought deliverance from the concerns of everyday life in avoiding marriage and children (Randall 1970, pp. 24–31), while the Pythagoreans tried to escape from sexual desire, which they considered evil (Bullough 1973, p. 107). To bring the passions under the control of reason, Hellenistic cults and philosophers adopted programs of self-imposed deprivation and hardship. In the 1st century A.D., Epictetus sought to accomplish the same goal through the voluntary repression of desire (Fairweather 1924, p. 18; Hatch 1957, p. 147).

The repudiation of pleasure because it prevented the attainment of salvation or distracted the mind from philosophical contemplation did not necessarily entail total sexual abstinence. A neo-Pythagorean treatise of the 1st century B.C. permitted sexual intercourse for procreation, though not for pleasure (Bullough 1973, pp. 109–10; 1975, p. 170). Other contemporary writings, including those of Philo of Alexandria, stated similar views. An immediate corollary to Philo's rejection of sexual pleasure (as well as to his neo-Platonist dualism, which was hostile to effeminacy in men) was a sharply negative view of homosexuality.¹²

As governmental administration in the Hellenistic kingdoms and Roman Empire became bureaucratic and impersonal, and national or imperial in scope, city politics atrophied. By the 3d century A.D., private donations for public buildings dropped dramatically, a mark of a decline in civic spirit among the wealthy. A malaise set in among the Roman upper classes, paving the way for Greek philosophy, and so Stoic and neo-Platonist doctrines

¹¹ An additional factor, suggested to us by Nicky Spiller, may have been an intensification of male domination, which, according to Slater (1971), occurred shortly before the classical age. When male domination appears or is strengthened, the ideology justifying the sexual division of labor is likely to embody invidious dichotomies and equations, such as mental activities (= male, superior) vs. physical activities (= female, inferior). We note in passing that Hindu, Buddhist, and Persian sources for Greek dualism have been asserted by many scholars over the years but have never been established definitively.

¹² Boswell (1980, p. 128–30) points out that dualistic philosophies such as Stoicism did not necessarily require hostility toward homosexuality; in fact, dualism was compatible with amoral hedonism as well as with asceticism. That dualism was more often associated with ascetic practices must be attributed to the social context, which excluded hedonism for much of the population.

spread among the educated (Barker 1966, p. 11; Jones 1966, pp. 24, 121, 128, 279; Brown 1978, p. 48).

The old Roman religion based on agricultural rites declined in Rome itself (though not in the countryside) despite politically motivated governmental efforts to revive it. Oriental mystery religions won converts by promising immortality and union with God through initiation rituals and personal purity. Denial of the body and its impulses was a major dimension of purity in these religions. By 100 A.D., ascetic currents were visible features of Roman religious culture (Randall 1970, pp. 100–106).

The civil wars and invasions of the 3d century can only have strengthened these ascetic tendencies, but the precise influences of these events have never been traced in detail. However, loosely organized groups of ascetic aristocrats leading lives of chastity and prayer could be found in Rome before A.D. 300 and are known to have existed throughout the 4th century (Rousseau 1978, pp. 80–81). The renewed military threat posed by the Goths' successful invasion of Rome and seizure of Aquileia in 401 greatly enhanced the appeal of ascetic versions of Christianity and led to a flurry of conversions (Rousseau 1978, pp. 80–81, 90).

Early Christian views of sexuality were shaped by both Jewish and Greek thinking. The Hellenistic influence, already important in Judea before the Roman conquest, became even stronger afterward and was one of the most important influences on early Christian theology (Fairweather 1924, pp. 220–301; Jones 1966, p. 26).

The economic crisis of the 3d century gave asceticism a wider appeal in the Empire's eastern provinces, particularly in Egypt and Syria. The heavy burden of taxation and the sharpening of class divisions in peasant villages played havoc with middle-class aspirations. Ascetic doctrines that advocated sexual abstinence and withdrawal from worldly affairs appealed to this class particularly by offering a strategy for coping with desires frustrated by uncontrollable external conditions (Vööbius 1960a, p. 120; Brown 1978, pp. 31, 82–85).

The most prominent themes in the Christian ascetic literature of this period reflect these experiences. Essays in praise of virginity, written for male readers, expand on the cares and worries associated with the support of wife and children. Emphasis is placed on curbing anger and controlling the appetites for food and sex. In tones of unrelieved pessimism, the ascetic literature portrays the world as a vale of suffering and disappointed hopes. Secular sources of happiness are described as inevitably transitory. Deliverance was attainable only through renunciation of the world and oneself (de Mendieta 1955; Saint Gregory of Nyssa 1967; Brown 1978, pp. 86–88).

The influence of these ideas appears to have been wide. According to a chronicler of asceticism in the Syrian Orient, in the latter part of the 4th century "great masses turned to the monastic life, contributing to the de-

population of the communities in the villages and towns, a process which took on steadily growing dimensions" (Vööbius 1960a, p. 122). It is estimated that in Egypt as many as 2% or 3% of the population may have belonged to ascetic Christian monasteries (Brady 1952, p. 92). Although the bulk of the converts to Christianity were from the lower middle classes who were hardest hit by economic catastrophe (Jones 1966, p. 26; Knowles 1969, p. 14), some of the most prominent came from well-to-do families. The contrast between the certainty of salvation and the uncertainties of secular political careers figured in the conversions of Ambrose, Jerome, and members of Augustine's circle, among others (Rousseau 1978, p. 93).

Early Christian writings were generally antagonistic to homosexuality. However, this antagonism was not restricted to Christian thought. Although John Chrysostom was among the most vehement of the early fathers of the church in his denunciations of homosexuality (Boswell 1980, pp. 359–60), his somewhat older contemporary Libanius, a pagan born in Antioch, expressed similar feelings with equal forcefulness (Jones 1961, p. 972). Boswell notes that in the West, the Christian church was the only organized entity to survive the German invasions. It thus became "the conduit through which the narrower morality of the later Empire reached Europe. It was not, however, the author of this morality. The dissolution of the urban society of Rome and the ascendance of less tolerant political and ethical leadership occasioned a steady restriction of sexual freedom which transcended credal boundaries. . . . All the organized philosophical traditions of the West grew increasingly intolerant of sexual pleasure under the later Empire, and it is often impossible to distinguish Christian ethical precepts from those of pagan philosophy during the period" (1980, pp. 127–28). We differ with Boswell only in seeing economic and political developments, rather than the deurbanization they occasioned, as the precipitants of these trends.

Moreover, Christian hostility to homosexuality was not directed against that alone, but toward all forms of sexual activity. Although the New Testament did not look favorably on sexual expression,¹⁸ the leaders of the early church gave sex much greater attention and rejected it far more passionately and completely. Virtually all the church fathers—Gregory of Nazianzus, Gregory of Nyssa, John Chrysostom, Ambrose, Augustine, Jerome—praised virginity and looked on sex with horror (de Mendieta 1955; Dodds 1965). Tertullian regarded unchastity as worse than death (Bullough 1973, p. 98). In the early Syrian church, only unmarried Christians could be baptized (Vööbius 1951), and some Western bishops of the second

¹⁸ Rev. 14:4 describes a procession of the redeemed as consisting of virgins not defiled with women; in a more influential passage in 1 Cor. 7:12, Saint Paul advises that "it is better to marry than to burn." Celibacy was better than marriage, but the latter was clearly preferable to sin, and thus was not sinful in itself.

century made continence compulsory for church members (Grant 1970, p. 271). A number of Christian writers, including Eustathius of Sebastia, a bishop of the mid-4th century, held that married people could not be saved. Augustine considered sexual pleasure within marriage to be sinful, even though intercourse was nonetheless redeemed by the desire for children and by the sacramental character of marriage (Erickson 1976). The 2d-century apocryphal Acts of the Apostles maintained that married persons should refrain from sex¹⁴ (Davies 1980, pp. 32–33), and “spiritual” (celibate) marriages were not uncommon among early Christians.

A number of the Gnostic versions of Christianity held equally negative views of sex. The Marcionite communities in Persia and Mesopotamia were celibate, and the followers of Valentinus—the best known of these was Origen—castrated themselves. This practice became common in Syria and Mesopotamia (Vööbius 1951, pp. 15–16).

As the Christian church was uncentralized administratively prior to the conversion of the Emperor Constantine in 313, no single doctrine of sexual conduct prevailed throughout the entire church. Instead, each bishop exercised absolute authority in his own diocese, influenced only by the moral authority and persuasiveness of his fellow bishops. However, by the middle of the 3d century, church leaders began to oppose excessively rigid discipline, knowing that it would hinder recruitment and lead to the loss of members (Grant 1970, p. 221). By the 4th century, church councils forbade self-castration (Bullough 1973, p. 100), and by the late 4th to early 5th centuries, perfectionism, including sexual abstinence as a requirement of church membership, had been clearly rejected. The policy that emerged was one of accepting sexuality only within marriage.

As Bullough (1973, p. 111) points out, the early church competed with rival sects, including Gnostic cults, for members. The success of the orthodox church may have been due partly to the way it reconciled the public demand for asceticism and personal purity with the necessities of organizational survival. The Gnostic sects' requirement that all members remain celibate might have discouraged potential adherents and if practiced by orthodox Christians would have prevented the growth of the sect by member procreation. In contrast, the orthodox requirement of sexual abstinence only for a clerical elite facilitated organizational growth and at the same time provided an ideological basis for hierarchical authority within the church. The establishment of confession and penances also helped to retain members, structure lines of authority, and evoke feelings of gratitude when violations of rules imposed by the church itself were forgiven.¹⁵

¹⁴ In addition, these works condemn the consumption of meat and wine and the ownership of personal possessions (Davies 1980, p. 12).

¹⁵ We are indebted to Caroline Persell for this last observation. Further speculation about the factors that contributed to the church's organizational success can be found in Pagels (1979).

That Christian opposition to homosexuality reflected a broader rejection of all sexual experiences not intended to lead to procreation within marriage is evident in the even-handed treatment of heterosexual and homosexual offenses in the writings of the early church fathers. For example, Saint Basil of Nyssa, the founder of Christian monasticism, wrote in 375 to another bishop, "He who is guilty of unseemliness with males will be under discipline for the same time as adulterers." Saint Gregory of Nyssa explains the reason for this in a canonical letter written to the bishop of Melitene in 390: both heterosexual adultery and homosexual intercourse are unlawful pleasures (McNeill 1976, p. 79; Gauthier 1977).

The canons adopted in 309 by the Council of Elvira (now Granada) make clear that these views were not idiosyncratic. So far as is known, Elvira was the first council to formulate canons for the regulation of sexuality; 37 of the 81 canons adopted concerned this topic. The one dealing with homosexuality specified that men who engaged in sexual relations with boys should not be admitted to communion even at death.¹⁶ Other canons specified the same rigorous penalties for adulterous women and women involved in pandering and prostitution (Laeuchli 1972, pp. 126-35). A number of the early penitentials also prescribe the same penances for homosexual and heterosexual offenses.¹⁷

Roman criminal law reflected a repressive stance toward homosexuality even before the Empire became Christian. By the 3d century, male homosexual prostitution was illegal in the West (Bullough 1976, p. 332; Boswell 1980, p. 170). Further legislation was adopted in the 4th century. The precise scope of this legislation remains a matter of debate, but it is clear that some forms of homosexual activity had become capital offenses by the end of the century.¹⁸ Legislation promulgated by Justinian in the early 6th century reiterated the death penalty for male homosexual repeat offenders.

¹⁶ Not too much should be made of the restriction of the prohibition to boys. The Council was not legislating a general code for sexual conduct; it was taking up concrete cases that had been presented to the Spanish bishops (Laeuchli 1972, p. 101). Keith Thomas (1980) observes, Boswell notwithstanding, that most homosexual relationships involved adult-juvenile pairs.

¹⁷ This is true of the 6th-century Book of David and the 7th-century penitentials of Cummean and of Theodore of Tarsus, Archbishop of Canterbury. However, the 7th-century Penitential of Columban and the 8th-century Burgundian Penitential treated homosexual infractions more severely than heterosexual ones (McNeill and Gamer 1938; Bullough 1976, p. 361).

¹⁸ Bailey (1955, p. 70) suggests that a law promulgated by the Emperors Constans and Constantius (sons of Constantine) in 342 may have been "facetious" but does not explain why a law with facetious intent would have been issued; Boswell (1980, p. 123) holds that the law outlaws homosexual marriages only and Lauritsen (1981) that it deals with all male homosexual acts. Similar disagreement surrounds the scope of a statute of the emperors Theodosius, Valentinian, and Arcadian. Boswell (1980, p. 124) claims that it refers only to "forcing or selling males into prostitution," while Bullough (1976, p. 332) and Lauritsen (1981) see it as a more general condemnation of passive

For our purposes, the precise scope of this legislation is secondary; the extent to which the general population shared the broad sentiments of the legislation is of greater interest but not easy to ascertain. The response of the population of Thessalonika in Greece in A.D. 390 to the arrest of a popular charioteer¹⁹ on homosexuality charges (they rioted and killed a Gothic officer of the Empire) is often taken as evidence that the edict of Theodosius flew in the face of popular sentiment (Crompton 1978; Lauritsen 1981), but we do not know that feelings were the same everywhere in the Empire, or indeed that the Thessalonikans would have responded similarly had the arrest involved a less popular figure.

The veneration of ascetics tells us that awe of sexual abstinence went beyond a tiny Church elite. Had there been a gross disparity between Christian ideals and popular values, conversions to Christianity before it became the official religion of the Empire would be puzzling. At the same time, the wording of the canons adopted at Elvira suggests that the bishops and presbyters realized many Christians were not living up to ideal Christian standards (Laeuchli 1972, pp. 101–2). We have no reason to think that the masses were obsessed with homosexuality. Even in the Patristic literature, it is not given great prominence. And the fact that the legislation of the 4th century was not enforced (a tax on male prostitutes was collected until the end of the 5th century) is good evidence that the repression of homosexuality was not a high priority for the government.

Since all Roman antihomosexual legislation from the 4th century on was introduced by Christian emperors, it has generally been assumed to have reflected Christian attitudes toward homosexuality. Boswell (1980) has criticized this assumption, arguing that Christians were unlikely to have been particularly antagonistic to homosexuality. His critics (Johansson 1981; Lauritsen 1981) have attempted to refute Boswell's arguments by producing evidence that the church was indeed hostile toward homosexuality, but they do not show that Christians were more intolerant than pagans of the same class. We simply do not know enough about attitudes among different strata or in different parts of the Empire to clarify this point.²⁰ What we do know is that homosexuality was not the only sexual

homosexuality. Of the two surviving texts for the act, only one mentions male brothels, and it is not evident that the reference is to coercion (see the translation Lauritsen provides, as well as Bullough [1976], p. 332).

¹⁹ The episode is usually assumed to have involved consensual homosexual activity and the arrest to have been a result of the Theodosian Constitution. But King states that the episode involved a rape of the Gothic officer and points out that it is uncertain whether the arrest came before or after the legislation (1960, pp. 68–69, 102–4). If the episode was in fact a rape, popular outrage at the arrest might have reflected antagonism to the Goths rather than acceptance of homosexuality.

²⁰ One might wonder why, if antihomosexual legislation was strictly religious in origin, it was not repealed by Julian the Apostate. During his short reign he attempted (though with very limited success) to undo the Christianization of the Empire.

offense singled out for severe treatment in late Roman legislation. Adultery was punished severely under the Antonine emperors (who were pagans) as well as under their successors; in the Justinian Code it was a capital offense (Laeuchli 1972, pp. 92-93; Boswell 1980, p. 171).

The Imagery of Homosexuality

Increasing intolerance was only one element of the shift in perceptions of homosexuality during late antiquity. For the Stoic and Epicurean philosophers, love and lust were deplorable distractions from the *vita contemplativa* but had no profound moral connotations. The Patristic literature of the 4th and early 5th centuries shifted the focus by making virginity a major component of personal purity. Sexual abstinence replaced martyrdom as the prescribed means of imitating Christ (Malone 1950). Justinian's language is quite different. In his legislation, homosexuality threatens not the individual sinner but the entire community. His Novellae of A.D. 538 and A.D. 544 attribute earthquakes, famine, and pestilence to homosexuality (Bullough 1976, pp. 171-72).

These patterns are exactly those predicted by Mary Douglas's (1970) neo-Durkheimian analysis of the relationship between social organization and body experience. Douglas postulates a correspondence between the ways people experience their bodies and the ways they experience society. She goes on to conceptualize society in terms of two variables, group and grid. *Group* refers to the strength of identification with a group, *grid* to the existence and stability of formally recognized, differentiated social roles. Each of these variables can have high or low values in a given society or other collectivity.²¹

Where grid is high, Douglas argues, we should expect an affirmation of society and its institutions. Where, in addition, group is low, religious syncretism would be expected (since ideological boundaries that would stand in the way of external religious influences would be weak). Because formalized social roles imply that public behavior is generally subject to social control, we would not expect sexual expression to be uninhibited. But neither would we expect it to be denied or repressed altogether. Instead, we would expect it to be institutionalized through ritual, as in fertility ceremonies. This seems like a good description of the polytheistic agricultural societies of the ancient Near East and perhaps of Greece and Rome in their early stages.

Where group and grid are both weak, religion loses its social character and magical qualities and becomes personal. So does morality. The uni-

²¹ In the paragraphs that follow, we attempt to summarize Douglas's conclusions as best we can given the ambiguities of her text. In addition, we try to spell out more concretely than she the logical implications for sexual ideology of her analysis.

verse comes to be seen as benign (or, possibly, indifferent). The blurring and instability of social roles would lead to the weakening of social control over sexuality. Where the universe is conceived of as benign, individuals would feel relatively uninhibited sexually; where the universe is considered indifferent, sexuality may be subject to self-regulation but can take many forms. Classical and Hellenistic Greece, and Rome under the early Empire, provide reasonably good examples.

Where group is high and grid low, the most important social distinction is whether someone is inside or outside the group. Dualistic philosophies, ascetic life-styles, and doctrines of personal purity will prevail. A preoccupation with what goes in and out of the body will parallel the social concern with group boundaries. Here one will find deep personal anxieties connected with diet and sexuality. Lapses will be treated as sins and associated with the powers of evil in the universe.

The Christian church of the 3d and 4th centuries corresponds well to this description. Both persecution and the need to differentiate the new cult from paganism, Judaism, and heresy made group membership extremely important. According to Brown (1972, p. 134), "The Christian communities in the third and fourth centuries had grown up in precisely those classes of the great cities of the Mediterranean that were most exposed to fluidity and uncertainty." Indeed, Christians had an exceptionally high degree of mobility (Brown 1972, p. 135). And during these first few centuries, lines of authority in the church were still fluid and ambiguous. Other sources of identification—such as kinship, race, or town—were downgraded systematically within the church, which sought to transcend these categories (Brown 1978, pp. 56, 74).

Last, when grid and group are both high, we expect a "complex, regulative cosmos." Body control will be important because fixed social roles will be accompanied by strong social control. However, there will be less concern with intentionality or subjective states of mind and more with overt, visible behavior. Instead of ascetic doctrines of self-discipline and personal striving for spiritual perfection, elaborate ritualized codes will govern behavior. Violations will be seen less as personal blemishes and more as threats to the collectivity. The well-being of the entire community is jeopardized by the misbehavior of individual members, and as a result the deviant is conceived of as an outsider, an alien.

This is precisely the pattern seen in the Justinian Age. Two centuries earlier, Diocletian had attempted to fix people in their occupations and bind cultivators to the soil. In the West, barbarian invasions had led to the breakdown of an organized, imperial society; but in the East, where defenses against the invasions had been more successful, society remained "firm and well-ordered" (Barker 1966, p. 13) though threatened by border conflicts on several fronts. Disastrous earthquakes, droughts, fires, floods,

epidemics, and visitations of locusts, leading to destruction on an immense scale and enormous loss of life, added to the sense of personal insecurity (Vasiliev 1950, pp. 344–50). As noted earlier, the Justinian legislation explicitly linked homosexuality with these natural disasters. Regarding himself as responsible for the fate of the Eastern Empire, Justinian also enacted harsh legislation against heretics and non-Christian religion. His Novella 77, which proscribed homosexuality, also forbade “swearing and blasphemy” (Ure 1951, p. 62). In an attempt to earn God’s favor and bring prosperity to the Empire, he forbade clerics to gamble or go to the theater or the races. These laws and prohibitions cannot be dismissed as the idiosyncrasies of a caesaropapist ruler; the belief that “natural” disasters were a divine response to neglect or disobedience was extremely widespread among both pagans and Christians in the late Empire (Jones 1966, p. 321).

Although Douglas’s scheme appears simplistic and mechanical in its equation of bodily and social experience, it explains the sexual ideologies of late antiquity so well that we must take it quite seriously, at least as the starting point for further investigation.²²

FEUDALISM AND HOMOSEXUALITY

Following the collapse of the Roman Empire in the West, there is little reference to homosexuality in secular sources. With a single exception, the *Leges Barbarorum* of the German nations do not mention the subject.²³ Since the Germanic peoples probably did not stigmatize homosexuality, this is no surprise (Bremmer 1980; Boswell 1980, pp. 183–84). Alcuin, the 8th-century Anglo-Saxon scholar, deplored incest and adultery but not homosexuality. Apart from a capitulary of Charlemagne that imposed no penalty (Bullough 1976, p. 353; Boswell 1980, p. 177), secular legislation dealing with homosexuality simply does not exist. In sacred law, as previously noted, the penitentials often treated homosexuality on an equal footing with heterosexual offenses. Moreover, Boswell (1980, pp. 179–83) indicates that in practice penalties were often mitigated and that the penitentials themselves were not widely used. While some such officials were unequivocally

²² We note particularly the absence of any conception of class differentiation in the structuring of social experience (such as we argued is relevant to an understanding of the growth of asceticism in the Roman Empire of the 4th century), or of external anxiety-producing threats, such as occurred in 6th-century Byzantium. The medicalization of sexual deviance in late-19th-century England and America seems to lie outside the framework of the scheme.

²³ The exception is the Visigothic Code in Spain. This code reflects greater Roman influence than any of the other barbarian codes. For evidence that this Code does not simply reflect the influence of the Church, see Bachrach (1973) and Boswell (1980, pp. 174–77).

hostile to homosexuality,²⁴ there is reason to think that these sentiments were far from universal, even among the clergy.

Although we cannot be overly confident about conclusions based on very limited evidence about a period with few extant records, it seems unlikely that homosexuality was repressed vigorously during the early and high Middle Ages in western and central Europe. Latin poets of the 9th to 12th centuries wrote un-self-consciously of their homoerotic attachments (Herman 1976; Curtius 1953, pp. 114–16; Allen 1928, pp. 149–51). Many of these were monks (Boswell 1980, pp. 188–96). In early 12th-century England, Saint Anselm urged that ecclesiastical penances for homosexuality be moderated because “this sin has been so public that hardly anyone has blushed for it, and many, therefore, have plunged into it without realizing its gravity” (Ellis 1936, p. 40; Hyde 1970, p. 35). The prelate Jacques de Vitry described the city of Paris in 1230 as filled with sodomites (Karlen 1971), while in 13th-century Parma the nobleman Salimbene di Adamo wrote that homosexuality was common, particularly among clerks and scholars, as well as among nuns (Cleugh 1963, p. 91; Karlen 1971).

This comparative acceptance of homosexuality can be understood in the context of the medieval social order. The developments that had given rise to asceticism in late antiquity were now long past. The collapse of the Roman Empire had had a totally different meaning for the conquering German nations than for the defeated Roman population. The bureaucratic administration of a vast empire had been replaced by local government. The emerging decentralized feudal society had left the population better able to defend itself against the Norse, Hungarian, and Arab invasions and therefore less prone to withdrawal into passive asceticism. The urban lower and middle classes who had suffered in the economic collapse of the 4th century ceased to exist in a society of self-sufficient manors.

When social survival depends on combat carried on by men fighting as individuals or in small groups, as was the case in medieval warfare, personal traits associated with hand-to-hand military combat and its associated ethical code are culturally elevated and acquire erotic significance for men as well as women.²⁵ Bullough (1973, p. 165) notes that in the song of Roland, women appear only as shadowy, marginal figures: “The deepest

²⁴ Writing in the last half of the 9th century, Benedict Levita forged a capitulary of Charlemagne that gives the penalty for sodomy as burning (Boswell 1980, pp. 177–78, n. 30).

²⁵ According to C. A. Tripp (1975), similar processes are found in primitive societies. He indicates that in societies where the prowess of individual men in hunting or war is praised, the associated traits become eroticized and male homosexuality is found. On the other hand, in those societies where these male activities are carried on collectively and the participation of individual men receives no special recognition, homosexuality is absent, whether or not the culture proscribes homosexuality. Unfortunately Tripp fails to specify the sample of societies studied, the anthropological sources he consulted, and the coding procedures utilized in the analysis.

signs of affection in the poem, as well as in similar ones, appear in the love of man for man, the mutual love of warriors who die together fighting against odds, and the affection between the vassal and the lord or within the church, between two clergy, usually an older and younger."

The arrangements for training knights would have tended to encourage homosexuality. Bullough (1976, pp. 399-400; see also Duby [1968] and Verbruggen [1977], pp. 28-29) notes that "usually, the young noble youth was incorporated into a group of friends who were taught to love one another as brothers, who were led by an older man, and whose every waking moment was spent in each other's company. Sometimes these groups stayed together for as long as 20 years, from age 11 or 12 until 30 or so, when they were supposed to marry. Sometimes marriage was further delayed, for eligible women were not particularly plentiful." According to Duby (1977, p. 115), within these groups "morals were far from strict. . . . When Roger and his companions left the household of Chester . . . [in northwest France of the 12th century] Ordericus Vitalis describes them coming back *Quasi di flammis Sodomiae*." Duby notes that other contemporaries also portrayed these groups of youth as having "depraved habits."

In addition, the monastic orders of knights (such as the Templars and the Teutonic Knights) were sworn to strict chastity. Although the early 14th-century accusation linking the Templars with rampant homosexuality may have been fabricated, the association between homosexuality and individual knightly combat elsewhere (Japan before the Meiji restoration, classical Greece, the military courts of the amirs in the Middle Period of the Islamic world) makes it plausible that homosexuality was common and accepted in the early and high Middle Ages among knights and nobility in Europe and England.

Since the relationship between a lord and his vassals was personal (the ceremony of homage and fealty culminated with a kiss on the mouth between lord and vassal), it would have been strengthened, not weakened, by erotic attachment.²⁶ Thus there would have been no need to discourage homosexuality in order to avoid problems for the dominant forms of social relations.

Boswell (1980, pp. 207-66) demonstrates that an urban-based male homosexual subculture apart from the knightly classes flourished in the towns of the 11th and 12th centuries, particularly, but not exclusively, among clergy and university students (who were often clerics). We will comment below on the reasons for this association between clergy and homosexuality.

²⁶ This contrasts with bureaucratic forms of domination, which are expected to be impersonal and universalistic. Sexual relationships within the bureaucracy—whether heterosexual or homosexual—might tend to interfere with the universalism of bureaucratic decision making. The medieval French tale of the love of two knights, Amis and Amile, which Boswell (1980, pp. 239-40) summarizes, is consistent with our claim.

Ecclesiastical denunciations of homosexuality began to reappear in the 11th century, with homosexuality among the clergy becoming a target of persistent criticism. Peter Damian, the energetic church reformer, led the attack with his mid-century castigation of ecclesiastical sinfulness, the *Liber Gommothianus*, which urged Pope Leo IX to impose the maximum penances allowed in the penitentials for all homosexual violations regardless of age and circumstances—a proposal that Leo rejected. In Alain of Lille's 12th-century *De Planctu Naturae* (*The Complaint of Nature*), the goddess Natura condemns homosexuality along with other sexual vices, and in the following century, Scholastic theologians characterized homosexuality as a sin "contrary to nature" (Bullough 1976, pp. 378–82; Boswell 1980, pp. 303–32).

In an atmosphere of growing hostility, the Council of London (1102) decreed that clerics guilty of sodomy were to be degraded, while those who did not desist were to be anathematized. The Council of Nablus, held in the Kingdom of Jerusalem in 1120, specified that sodomists be burned, although the penalty was mitigated for those who renounced the vice voluntarily, and canons with less rigorous penalties were adopted at the Third Lateran Council in 1179. Provisions derived from those of the Lateran Council appear in the local legislation of a number of 13th-century synods (Bullough 1976, pp. 383–84; Goodich 1979, pp. 41–46; Boswell 1980, p. 281).

Beginning in the mid-13th century these sentiments were embodied in secular legislation, which betrays knowledge of the recently revived Justinian Code by its prescription of the death penalty for sodomy.²⁷ Thus a customal (collection of customary law; its provisions were not always traditional) prepared in Orleans around 1260 required that third offender sodomists be burned. Similar penalties are indicated in Beaumanoir's *Les Coutumes de Beauvaisis* written in 1283 (Philippe de Remi 1842, p. 413), in the *Très Ancien Coutume de Bretagne*, the *Coutumes de Touraine-Anjou*, and the forged *Etablissements* of Louis IX (Goodich 1979, pp. 77–78).

²⁷ The meaning of the term "sodomy" is usually not spelled out in detail in this legislation, but often went beyond homosexuality. A 15th-century handbook for confessors includes male and female homosexual intercourse and heterosexual intercourse "outside of the fit vessel" in its definition of sodomy (Bullough 1976, p. 381); in the 16th century, the Roman penologist Farrinaci reports that married couples were burned at the stake for sodomy (Becker 1964, p. 119). Heterosexual sodomy was prosecuted in early 16th-century Florence, and preachers denounced this practice regularly (personal communication from Gene Brucker). Men tried for bestiality in 16th- and 17th-century France were also prosecuted as sodomists (Hernandez 1920). The close association between religious heresy and sodomy in some of the codes (see, e.g., Philippe de Remi 1842, 2:413) raises the possibility that the authors of the code intended the death penalty only for those cases of sodomy in which the perpetrators were involved with heretical movements. In France this association had been established with regard to the Albigensians, who shunned procreation but may have tolerated forms of intercourse with no potential procreative result (Russell 1965, p. 203).

Parallel developments are traceable outside France. The late 13th-century *Las Siete Partidas*, promulgated by Alfonso X of Castile and Leon, prescribed castration followed by stoning to death for homosexuals (Karlen 1971, pp. 89, 289); Ferdinand and Isabella changed the penalty to burning in 1497. In England, two treatises dealing with the administration of royal law dating from around 1290 assert that sodomy should be punished by death; *The Mirror of Justices*, attributed to Andrew Horn, specifies burying alive as the penalty, as does *Fleta*. However, neither work is entirely trustworthy.²⁸

In addition to this body of royal legislation, the municipal statutes of the communes in Northern Italy in the 13th century specify rigorous penalties for sodomy (Goodich 1979, pp. 79–83). Although antisodomy legislation was generally not enforced vigorously,²⁹ prosecutions and executions were carried out sporadically in the late Middle Ages outside England.

These developments can be attributed plausibly to two distinct but related sources: church-state conflict and class conflict. With regard to the former, we will argue that the growing preoccupation with homosexuality was an indirect and partly unanticipated consequence of the attempts of church reformers in the mid to late Middle Ages to establish sacerdotal celibacy. The psychological conflicts engendered by forced celibacy, we suggest, resulted in an irrational hostility toward homosexuality among both clergy and laity. With regard to the latter, we will contend (along with Goodich) that a popular hostility toward homosexuality was part of a

²⁸ Plucknett (1956, p. 267) describes the *Mirror* as "certainly the most fantastic work in our legal literature. The author knew some Anglo-Saxon laws (and fabricated many more). . . . The work never circulated during the Middle Ages, but was believed to have been genuine when discovered in the sixteenth century." In any event, the *Mirror*, while expressing horror at sodomy, also forbids bringing accusations to court. *Fleta* is a more useful work but still not trustworthy on every point (Plucknett 1922, p. 80; Ogg 1925, pp. 191–97). The work was not a code, as Goodich (1979, p. 77) and Boswell (1980, p. 292) seem to believe (Goodich also erroneously reports the penalty as burning), but a work of jurisprudence. It epitomizes Bracton and attempts to bring Bracton up to date by taking into account the legislation of Edward I, and draws on other contemporary writings, as well as on Roman law. The passage dealing with sodomy does not appear in Bracton, and there is no statutory basis for it, even though the statutes of Edward I are known to us. No sodomy cases are recorded in the voluminous *Notebooks* from that period or any later one. No other legal commentator refers to such a prohibition, and Hanawalt (1979, p. 4) reports that in the first half of the 14th century sodomy cases were handled in the church courts. The text of the 16th-century sodomy statute (25 Henry 8, c. 6, 1533) states that no one had been punished for this offense for a long time. These considerations suggest that the author of *Fleta* is in error about English legal practice of his time. However, our argument does not rest on this conclusion.

²⁹ For example, Bullough (1976, pp. 391, 410) notes the lack of evidence that anyone was executed or even convicted of sodomy in the secular French courts during the reigns of Louis IX, Philippe IV, and Philippe V, even though the *Etablissements de Saint Louis* specified the death penalty. In the Occitan, male homosexuality appears to have been a common feature of town life, ignored until the Inquisition initiated a wave of persecution in the early 14th century (Le Roy Ladurie, 1975, pp. 9–21, pp. 209–15).

broader middle-class morality that became increasingly forceful in its opposition to a life-style of luxury and excess as class divisions widened.

The mid-11th-century papacy was engaged in a struggle for political supremacy with the Holy Roman Emperor. The church reformers who led this struggle hoped to create a theocratic empire by tightening the organizational discipline of the church so that priests' loyalties would be owed to the church, undiluted by allegiances to secular authorities or by affection for wives or concubines. Doing away with lay investiture and priestly marriages was thus an item of high priority in church reform.

The tendency toward hereditary offices in the feudalism of the time added to the reformers' determination to end priestly marriages. If the priesthood had become a hereditary benefice, the church would have lost much of its control over priests (Lea 1884, p. 193; Brooke 1972, p. 72), just as, later, in 18th-century France, royal authority was frustrated by the endemic sale and inheritance of governmental offices. Thus, these measures were taken by church officials as part of a program to strengthen the hierarchical command structure.

Other considerations also lay behind the campaign for celibacy. The church was becoming wealthy, and its accumulation of riches would have slowed if priests married and transmitted property intended for the Church to their offspring (Macdonald 1932, p. 28; Lea 1884, p. 64).

To strengthen the moral authority of the church, reformers hoped to draw a sharper line between the moral purity of the priesthood and the less restrained—and less pure—lives of the laity. Clerical marriage tended to blur the distinctions between clergy and laity. However, the theology of the time was placing greater emphasis on the sacraments, and thus attention was drawn to the sacredness of the person who administered them. Celibacy was considered to increase the priest's resemblance to Christ (Brooke 1972, p. 73; 1975, p. 254). Popular discontent with clergy who were married, had concubines, or patronized prostitutes became sufficiently widespread to create a legitimacy crisis for the church that could be resolved only through a restoration of the early requirement—never formally abandoned but virtually neglected in practice—of priestly celibacy.⁸⁰ Indeed, the reformers expressed the fear that the church would be brought into disrepute by clerical immorality (Macdonald 1932, p. 29; Brooke 1975, pp. 237, 254; Little 1978).

In carrying out this program, Pope Leo IX presided over a Council at Mainz (1049) which condemned simony (the purchase of ecclesiastical offices) and priestly marriages (Lea 1884). Popes Alexander II and Greg-

⁸⁰ Clerical marriage had, of course, been prohibited much earlier, e.g., by Pope Leo I in the 5th century. However, the prohibition had been weakened by the passage of time. By the beginning of the second millennium, clerical marriage had become common, and some authorities explicitly permitted it or else qualified the prohibition (Macdonald 1932, p. 29; Brooke 1972, p. 84; 1975, p. 254).

ory VII ordered priests who were guilty of fornication not to say mass and excommunicated Henry IV in the struggle over lay investiture. Papal decrees against attending masses celebrated by clergy who were married or had concubines were issued in 1059, 1063, and 1074. The First Lateran Council reiterated this prohibition in 1139, and Pope Innocent III took up the issue again in 1215 (Russell 1965, p. 7; Brooke 1972, pp. 75-77; Moore 1977, p. 53). Sons of priests were declared ineligible for the clergy (Schimmelpfennig 1979). The mendicant orders (Dominicans, Franciscans) were created at the beginning of the 13th century to assist the papacy in carrying out these reforms.

The celibacy rule met with strenuous and sometimes violent resistance from clerics and their wives, to the point where some Italian bishops "did not dare to announce the decrees" (Schimmelpfennig 1979; see also Wishart 1900, pp. 182-86; Brooke 1972, p. 84). For this reason, enforcement was spotty and at first only partly effective. Yet by the latter half of the 13th century clerical marriage had been eliminated to a large extent (Brooke 1972, pp. 78-99).

The elimination of heterosexual outlets for priests as a result of the celibacy rule could only have fostered the development of homoerotic feelings. Sexual experience is not merely a form of tension release or a source of physical pleasure; it is also a way of establishing and maintaining emotional intimacy with others (Benjamin 1978). In some people—the proportion is not known, but is probably substantial—and in some circumstances, the psychological need for such relationships is stronger than the orientation toward partners of a particular sex. Thus when a group of people is deprived of the opportunity to satisfy the need for emotional intimacy heterosexually, some members of the group can be expected to seek the fulfillment of that need homosexually.⁸¹

This is especially likely to happen in single-sex milieus, where contact with members of the opposite sex is entirely cut off. High levels of homosexual behavior have, in fact, been associated with prisons, English boarding schools, and Christian, Tibetan, Buddhist, and Islamic monasteries, as well as with some traditional Moslem societies, the classical Greek city-state, and Puritan New England, where heterosexual contacts were suppressed or curtailed (Lea 1884; May 1931, p. 247; Prince Peter 1963, p. 458; Brunel 1955, pp. 203-4).

The association between homosexuality and Christian monasticism dates back to the early history of coenobitic communities in late antiquity. Ref-

⁸¹ We are not claiming that all or even most homosexual behavior reflects the absence of opportunities for heterosexual outlets. However, it is implicit in our position that choice of sexual partners is not always fixed permanently in early childhood but is subject to social influence in adolescence and adulthood. It follows that where homosexual attachments are strongly discouraged we will find a correspondingly higher level of heterosexual involvements.

erences to homosexuality are almost completely absent from the earliest monastic literature, but in subsequent generations, measures to curb it received increasing attention (Bury 1923, 2:412; Vööbius 1960*b*; Chitty 1966, pp. 66–67). This pattern suggests that it was the monastic environment itself that evoked high levels of homosexual attraction among the monks, not selective recruitment (which, given the asceticism of the monastic movement, would have been unlikely).

The homophile poetry and letters of passionate male friendship and love written by 12th-century clergy to one another (Boswell 1980) can in part be attributed plausibly to the closing off of heterosexual relationships by the Gregorian reform movement. The attribution of homosexuality to university students of the time is equally understandable when we recall that medieval university faculty and students were clerics.⁸²

The church could tolerate priestly involvement in homosexual relationships no more than it could tolerate heterosexual involvement. Scripture was interpreted as condemning all homosexuality, and Patristic authority was unequivocal in its denunciation of homosexuality. The legitimation needs of the church would have been threatened by acceptance of any form of sexual expression on the part of the clergy, and the reformers' desire to build a priestly army unencumbered by personal or worldly attachments was inconsistent with homosexuality in its ranks.

It is only to be expected, then, that the extirpation of homosexuality among the clergy was on the reformers' agenda. Peter Damian, mentioned earlier for his vitriolic attacks on clerical homosexuality, was firm in his opposition to simony, clerical marriage, and concubinage (Brooke 1972, p. 72; Little 1978, p. 72). Anselm, who fought with William Rufus and Henry I over lay investitures, sought the Council of London's decree against homosexuality (Thomas 1980). Pope Innocent III, who continued Gregory VII's policy of seeking papal supremacy over the state, initiated an investigation into clerical sodomy in Mâcon in 1203 (Goodich 1979, p. 17). When Pope Gregory IX ordered a crackdown on clerical fornication in the German church in November 1231, he included those engaged in homosexual activity among those who were to be compelled to become continent (Little 1978, p. 142).

Despite the reformers' denunciations of homosexuality, their responses to it were often restrained. For clear political and organizational reasons, they gave much higher priority to ending lay investiture, simony, and clerical marriage than to the suppression of homosexuality. Thus, accusa-

⁸² The association between Catholic clergy and homosexuality persisted long after the period under discussion. Perry (1980, pp. 67–84, 122, 132) finds that in Seville around 1600 the practice of homosexual sodomy was popularly believed to be endemic in the priesthood, and clergy were represented disproportionately among those prosecuted for this offense. The government feared that cynicism about the immorality of the clergy would give greater appeal to heresy.

tions of homosexuality against church officials were sometimes ignored. Anselm urged in correspondence that "sodomites" not be admitted to the priesthood and wanted those already admitted warned against sin, but favored moderation in punishment (Goodich 1979, pp. 41-42). In many instances, clerics who were aware of their own homosexual impulses may have been reluctant to endorse or participate in the persecution of others (Boswell 1980, pp. 211-21); Peter Damian complained that those guilty of homosexual relations with priests could avoid serious penalty by confessing to their partners (Bullough 1976, p. 363).

Nevertheless, as time went on, the church became more uncompromising in its stance on homosexuality. This development followed almost inevitably from the success of the reform movement. The more the church suppressed priestly marriage and concubinage and strengthened monastic discipline, the stronger was the homosexual drive it must have aroused within its ranks. The organizational suppression of sexuality would have prevented many priests from giving expression to their homosexual impulses. The repression of sexual impulses that could be neither expressed nor acknowledged would have given rise to sharp psychological conflict. Fear and hostility toward homosexuality would have developed and increased in intensity throughout the Middle Ages as a psychological defense mechanism against this inner conflict.⁸³ The irrational and at times hysterical tones in which homosexuality is mentioned in the late Middle Ages can thus be understood as manifestations of reaction formation and projection originating in organizationally induced psychic conflict.⁸⁴

Gender and Class Conflict in the Middle Ages

Although the church devoted its greatest efforts to the control of homosexuality among the clergy, it was also concerned with the morals of the laity. Some of this concern might be expected from the indiscriminate na-

⁸³ Contemporary psychological research has established that heterosexuals are less accepting of same-sex homosexuals than of opposite-sex homosexuals, suggesting that these defense mechanisms against homosexual impulses continue to influence attitudes toward homosexuality (San Miguel and Millham 1976; Weinberger and Millham 1979). The connection between paranoia and anxiety connected with repressed homosexual impulses has been established in several studies (for a summary, see Kline 1972, pp. 265-75). Given the trend toward greater repression of homosexuality, it is a bit surprising to find as late as the end of the 15th century some reformers who still treated priests who took concubines and those guilty of homosexual offenses on an equal footing; yet they existed. Thus Savonarola complains in a letter that "this one goes at night to his concubine, the other to his youthful male lover, and then in the morning they go to say mass" (Lea 1884, p. 399). (The translation from the Italian is by David Greenberg.)

⁸⁴ The repression of priestly heterosexuality created conflicts that if anything were even more intense. This kind of conflict, reflected at first in Mariolatry, which flourished in the 12th century, and later in the witchcraft persecutions, contributed to the increasing misogyny of the later Middle Ages.

ture of psychological defense mechanisms; but the content of literature discussing homosexuality suggests that issues of life-style and gender stereotyping were as much at issue as sexual expression per se. For example, in late 11th- and 12th-century Normandy and England, the clergy complained about the prevalence of homosexuality in the royal entourage of the Norman rulers. The young men of the court had begun to wear long hair and women's clothing and adopted effeminate mannerisms. Thus the monk Orderic deplored the court of William Rufus, son of William the Conqueror, where "the effeminate predominated everywhere and revelled without restraint, while filthy catamites, fit only to perish in the flames, abandoned themselves to the foulest practices of Sodom" (Hyde 1970, p. 33). Similar passages appear in the writings of Orderic's contemporaries, including William of Malmesbury. In 1108 the Council of Westminster condemned men's wearing their hair long, and several leading churchmen not only preached against the effeminacy of the court but actually cut the hair of king and nobles with their own scissors (Tatlock 1950, pp. 351-53; Freeman 1882, 1:159; 2:330, 340-41).

These passages deplore homosexuality only incidentally; their deepest preoccupation is with men dressing and acting like women. In the upper levels of feudal society, sexual stratification had been extremely sharp. The new styles of clothing and bodily appearance cultivated by the post-Conquest Norman aristocracy were incompatible with the traditional pastimes of their class (hunting and military combat) and involved the imitation of members of a disvalued social status, women. It was this that the conservative clergy found repugnant.

Opposition to the luxurious and expensive standard of living of the court also appears prominently in writings about homosexuality among the laity. The following passage, written by the English cleric John of Salisbury in 1159, illustrates this theme:

When the rich lascivious wanton is preparing to satisfy his passion he has his hair elaborately frizzled and curled; he puts to shame a courtesan's make-up, an actor's costume. . . . Thus arrayed he takes the feet of the figure reclining by him in his hands, and in plain view of others caresses them and, not to be too explicit, the legs as well. The hand that had been encased in a glove to protect it from the sun and keep it soft for the voluptuary's purpose extends its exploration. Growing bolder, he allows his hand to pass over the entire body with lecherous caress, incites the lascivious thrill that he has aroused, and fans the flame of languishing desire. Such abominations should be spat upon rather than held up to view. . . . [Quoted in Karlen 1971, p. 87]

As this passage indicates, complaints about homosexuality began to be used to express popular discontent over a growing social gap between the court and the rest of the population. John was probably of modest social

origins and was alarmed at the erosion of traditional morality by the new wealth (Cantor 1969, p. 356).

Sodomy also was linked with wealth in the class conflicts that accompanied economic growth in the cities of northern Italy, northern France, Flanders, and the Rhine Valley beginning in the middle of the 12th century. In the first phase of the conflict, merchant capitalists (*popoli grassi*) supported by artisans, small tradesmen, and laborers (*popoli minuti*) contended with the aristocracy for political supremacy in city government. For the merchants, the cultural dimension of this conflict entailed a rejection of values and life-styles antithetical to their own. Whereas the bourgeoisie saw itself as industrious and self-reliant, it saw the aristocracy as lazy and parasitical (Cantor 1969, p. 412-14).

The bourgeoisie also reacted against sodomy (by which they meant all nonprocreative sexual activity) among the aristocrats, seeing it as an unproductive self-indulgence that expressed lust, not love or the desire for children. Indeed, sodomy became a metonym for excessive indulgence of material desires, evoking connotations that went far beyond sexuality. It represented a going beyond natural limits in the sexual sphere, in the same way that unrestricted greed did in the economic sphere.

Since the highest levels of the Italian aristocracy were involved in money lending (Moore 1977, p. 55), sodomy and usury quickly became linked in the popular mentality.³⁵ The historical association between the feudal aristocracy and homosexual sodomy makes this linkage unsurprising; what is probably more significant is the symbolization of usury in terms of the antispiritual amassing of wealth and the employment of it in ways that upset older forms of sociality and undermined traditional morality. In northern Europe, to attribute sodomy to the great Italian bankers who lent to the royal houses of England and Europe at high interest rates (Goodich 1979, p. 86; Koffler 1981) was to depict lenders who were "screwing" their debtors financially as doing so literally through sexual domination and exploitation of weakness.³⁶

By the mid-13th century, the bankers, merchants, and manufacturers who had ousted the old feudal nobility in the commercial centers came

³⁵ Dante, e.g., placed usurers and sodomists together in the same circle of Hell (Koffler 1979). Like others of his era, he used the term "sodomy" very loosely. For example, he characterized blasphemy and the refusal to write poetry in the vernacular as "spiritual sodomy"—the latter because the refusal to communicate violates the natural order (Pézar 1950, pp. 294-311).

³⁶ The widespread equation of sodomy and usury in the literature of this period suggests a possible revision of Mary Douglas's scheme, described above. When an economy becomes commercialized, it may be that it is not the experience of society as a whole that corresponds to the experience of sexuality but rather the experience of economic relations in particular. The importance of economic metaphors in Victorian literature on male sexuality (Barker-Benfield 1976) is consistent with this observation and suggests that it may be worth further exploration.

under political pressure from artisans and tradesmen who sought broader participation in city government. Heightened inequality of wealth exacerbated popular resentment of the profligate life-styles and vulgar ostentation of the *nouveau riche* capitalists. The *popoli minuti* raged against the economic practices of the great bankers and merchants (usury), and at the social and sexual habits they associated with these *popoli grassi*: gambling, adultery, prostitution, and sodomy (heterosexual and homosexual). The moral rigidity and fanaticism produced by the perilous class position of the popular strata can be seen in the fervor of their later campaign against opulence in the time of Savonarola (1490s), during which Florence enacted a new statute against homosexuality (Thompson 1959, 1:462-69; Mollat and Wolff 1973, pp. 288-89; Moore 1977, p. 55; Goodich 1976; 1979, pp. 9, 79-88).

Bourgeois hostility toward the medieval aristocracy was exceeded only by its antagonism toward the organized church. Clergy who lived in affluence on the contributions of the laity and whose personal morality was inconsistent with the claims of the church to a privileged status in spiritual matters were detested by an intensely pious bourgeoisie (as well as by the rural and urban poor). Donatism, the heresy that declares the invalidity of sacraments administered by immoral clergy, flourished in this atmosphere. Bourgeois demands to be free from compulsory church tithes added a material dimension to this cultural conflict (Herlihy 1958, p. 59; Moore 1977, p. 65). It was these nascent middle classes that demanded and supported the Gregorian reforms. Thus, the *popolo* attempted to overthrow the established church hierarchy in Milan, in alliance with the papacy. Non-celibate clergy were forced to flee the city when mobs beat them and plundered their homes (Brooke 1975, p. 343; Butler 1969, pp. 56-67; Schimelpennig 1979).

The Gregorian reformers considered the reconstitution of the spiritual life and organizational structure of the Church so important that to advance this goal, they were willing to countenance violent attacks on obdurate clergy. Geopolitical concerns reinforced this willingness. The Hohenstaufen ambition of subjugating all of Italy ran directly contrary to papal political interests, for, with Rome surrounded by the Holy Roman Empire, it could not have maintained its political independence. To prevent this, Rome supported the popular anti-imperial parties in the northern Italian cities. It was these parties that furnished the troops that, under the political leadership of Pope Alexander III, defeated the imperial armies at Legano in 1174 (Cantor 1969, pp. 430-31; Herlihy 1958, p. 55). Thus church-state conflict became implicated in urban class politics. The persecution of sodomists—directed largely though not exclusively against homosexual sodomy—was an indirect production of this confluence. The lay fraternities

created by the reforming friars (whose members, according to Freed [1977] were largely nonnoble) gave special attention to the persecution of sodomy.

By the end of the 13th century, the major elements in the Christian response toward homosexuality had been created. Scholastic theology had reconstructed sodomy as a sin against nature, far worse than other sexual sins. The mendicant orders (Franciscans, Dominicans) had been created with a special mandate to suppress heresy and sodomy. The Inquisition, staffed by these orders, had been established. Where it was given a free hand, as in Spain, it played an active role in the prosecution of persons suspected of homosexual activity (Perry 1980, p. 132). The governments of the revived commercial cities and the centralizing monarchies joined in this prosecution by executing offenders convicted in ecclesiastical courts and by trying, convicting, and executing offenders under their own secular authorities. Only in the modern era were these ideas and practices modified or abandoned.

DISCUSSION

Our analysis suggests that intolerance toward homosexuality and other forms of sexual activity grew in late antiquity because of the strains of profound social change. After a period of comparative acceptance, repression began again in the 13th century as an unanticipated consequence of organizational reforms in the church and of class conflict associated with the commercialization of medieval society.

Apart from clarifying the history of social responses to one particular form of sexual expression, our account has broader implications for deviance theory. The processes by which a given form of behavior comes to be stigmatized and subjected to repressive social control have been analyzed heretofore largely in terms of entrepreneurial social movements or of organizations seeking material or symbolic gain. Although such entrepreneurs appear in our account (e.g., the Gregorian reformers), our approach moves beyond this familiar level of analysis in a number of crucial respects. First, we examine the social-structural developments which give rise to these social movements and shape their goals. In doing so we do not neglect the ideological factors that influenced the character and direction of these social movements. An analysis that simply describes social movements and their goals and perspectives would shed light on these social-structural roots only to the extent that movement participants were themselves aware of these roots. We know of no reason to think that people are always aware of the experiences and constraints that limit and direct their thoughts and actions. The exploration of these hidden factors and their effects is a major task for sociologists, not only in the study of human sexuality, but more generally in sociology.

Moreover, in analyzing the role of social-structural developments, we do not restrict our gaze to economic factors. Although such economic developments as the growth of commerce, urbanization, and increased social stratification in the ancient world and the revival of a cash economy in the Middle Ages are important to our analysis, we also consider such morphological variables as the organizational structure of the church. An analysis that failed to consider the complex conjuncture of these variables would miss much that was important. In short, we caution against economic reductionism. Yet in stressing that the social creation of deviance-defining categories must be analyzed in relation to an institutional and ideological context, we are by no means advocating a functionalist explanation. We make no assumption that society has moral boundaries that are maintained by forbidding homosexual activity, that society as a whole gains by the prohibition, or that the patterns we have described are explained by their consequences.

It is an explicit feature of our work that we allow for the possibility of unanticipated consequences. The Hildebrandian reformers who campaigned for clerical celibacy do not appear to have expected that their success would later contribute to the execution of sodomists or witches, but our analysis suggests that the institutional reforms they carried out helped to bring about these results.

Second, we see the course of social change as being influenced not only by the direct intervention of participants in the social movements but also by members of the larger society from which movements recruit members and derive their ideas and whose acquiescence may be necessary if a movement is to succeed. The asceticism of the early church and the medieval reform movement were in part responses to popular demand, not solely impositions from the top.

Third, we have not considered homosexuality alone. The early Christian church was antagonistic, not just to homosexuality, but to wide varieties of sexual expression. It was this larger pattern that we attempted to delineate in our discussion of asceticism. Deviance theorists to date have largely neglected the relationship among responses to different kinds of deviant behavior;⁸⁷ our analysis suggests that the investigation of common patterns of responses may be a fruitful direction for further work.

Finally, we note that psychological processes occupy a place in our analysis. Institutional change can influence the socialization process and structure the opportunities and costs of satisfying psychological needs or drives. Although psychiatric writings on the etiology and treatment of homosexuality have been criticized widely for their antihomosexual bias, psychoanalytic theory may still contribute to an understanding of responses to homosexuality or other forms of deviance. For the most part, sociological

⁸⁷ One of the rare exceptions is Rothman (1971).

work in this area has relied implicitly on a "goal-directed rational actor" model of cognition and action. Useful though this model may be as a starting point, it is also psychologically shallow. Our work points to the possible transcendence of this limitation.

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An Empirical Typology of American Metropolitan Juvenile Courts¹

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The research reported in this paper develops a typology of American metropolitan juvenile courts through factor analysis of 96 court characteristics and a cluster analysis of courts based on indicators of five derived factors representative of structural dimensions. The data were collected through a mail and telephone survey of court personnel in a saturated sample of 150 metropolitan juvenile courts. The empirical typology is presented and its implications for future comparative studies and studies testing differential processing are discussed.

American juvenile courts are frequently portrayed as organizations characterized by informality of procedure, limited structural differentiation, and dedication to the goals of treatment and rehabilitation. This imagery is reflected in much of the prescriptive and normative literature that attempts to set standards for the courts and related agencies (e.g., Mack 1909; Lou 1927; National Council of Juvenile Court Judges 1967). It is also suggested in critical research that works from an ideal type to advance theory

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(Matza 1964; Packer 1968; Platt 1969). It cannot be assumed, however, that there is a single, uniform system of juvenile justice. Intensive, ethnographic studies of single courts (Cicourel 1968; Emerson 1969) have detailed the operations of individual systems, but they cannot be generalized to a larger population of courts and have done little to dispel the idealized image of the juvenile court.

Comparative studies of juvenile courts (Lemert 1967; Lefstein, Stapleton, and Teitelbaum 1969; Stapleton and Teitelbaum 1972; Cohen 1975; Hackler, Brockman, and Luczynska 1977; Cohen and Kluegel 1978, 1979; Figueria-McDonough 1979) and critical legal and sociological analyses of juvenile justice and juvenile law (Handler 1965; Dunham 1966; Schur 1973; Allen 1976; Tappan 1976) have drawn attention to the conflicting multiple goals and procedures of juvenile justice. Typologies representing differing philosophies and systems have been proposed, variously labeled casework-legal (Handler 1965; Tappan 1976), therapeutic-due process (Cohen and Kluegel 1978), informal-formal (Dunham 1966), cooperative-adversary (Stapleton and Teitelbaum 1972), and rehabilitative-punitive (Erickson 1974). At one extreme lies the system best described by the concept of *parens patriae*, with an emphasis on "helping" the child, intervening in his or her best interest. At the other lies the more formal, legalistic system, with a due process model of restricted information flow and precise rules of adjudication (Packer 1968). These attempts at classification are frustrated, however, by variations among jurisdictions (Matza 1964; Lemert 1967), which defy simple unidimensional classification.

Contradictions arising from research seeking the determinants of court dispositions in juvenile justice may well be explained by these variations. Studies reporting racial and class bias in juvenile court dispositions (Thornberry 1973, 1979; Thomas and Cage 1977) and those reporting partial effects (Arnold 1971; Carter 1979) have been challenged (Terry 1967; Cohen and Kluegel 1978; Wellford 1975; Tittle 1980) by research and critical reviews suggesting no such relationship. Refinements of problem situations and measurement classification for status offenses (Dungworth 1977; Carter 1979) and better definitions of discretion and discrimination (Horwitz and Wasserman 1980) have not resolved the issues.

The research reported in this article is an attempt to measure the variations in structure and procedure among juvenile courts by constructing an empirical typology that has implications for variations in juvenile justice procedures and the resulting outcomes of cases. It is our contention that the dominant value orientations of differing systems of justice are represented by observable structural correlates. We expect to find variations in value orientations and, thus, in the associated structural characteristics of courts within their juvenile justice context. These variations may reflect the ideal, polar types suggested in previous literature, and we hypothesize that

they represent not a continuum but points in multidimensional space. This research explores the dimensionality of juvenile court structure through factor analysis of court characteristics. The courts are then grouped on the derived dimensions through a cluster analytic procedure.

INSTRUMENT CONSTRUCTION AND DATA COLLECTION

The survey was designed to meet two separate, but not contradictory, objectives. The first was the development of a "users" checklist of common organizational features of juvenile courts of interest to juvenile justice practitioners. A stated purpose of the survey was to build a common data base from which judges, court administrators, and line personnel could draw information for cross-court comparisons.

The second goal of the survey was served by building into the questionnaire items related to contemporary court organizational theory. Explicit in this theoretical orientation was the view that courts may not be classical bureaucracies (Clynn and Neubauer 1981) with hierarchically organized rules and defined tasks. Of particular interest were the organizational framework posited by Eisenstein and Jacob (1977) and the concept that differing court organizational frameworks would reflect differing normative systems and work expectations (Stapleton and Teitelbaum 1972). These theoretical and empirical works suggested the inclusion of items to describe and measure variations in administrative organization (e.g., the nature and amount of coordination of the diverse activities of the court staff and related agencies), the division of labor (e.g., general and special case handling practices), and patterns of role relationships (e.g., complexity, formalization, differentiation).²

After the instrument was pretested on a selected subsample of courts, items relating to the scope of jurisdiction (whether or not the court was primarily concerned with status offenders) and the degree of discretion (as exercised by probation or intake services in the nonjudicial handling of cases) were added as potentially distinguishing among courts.

The final research instrument was an interview schedule of 78 closed and open-ended items that were separated into 203 distinct dichotomous variables. It represented the best effort to collect a parsimonious data set combining both practical and theoretical interests. The selection of items for the interview schedule was completed by convening a panel of juvenile justice practitioners as a "focus group" to judge the appropriateness and

² Despite the importance of size as a variable in organizational research, there are difficulties in measuring the concept with respect to courts. Possible indicators include number of personnel, case load, and number of individuals processed—each with its own limitations. Rather than include size as a variable, we attempted to control for its effects, at least to some extent, by restricting the sample to metropolitan juvenile courts. The generalizability of our results to smaller courts awaits further research.

wording of the individual questions. Specifically, questions were designed to elicit information about court jurisdiction, types of judicial officers, court services and their administrative structure, procedures to ensure due process, and decision-making procedures and options at intake, detention, adjudication, and disposition. For purposes of the typology construction, however, only those variables with theoretical relevance were selected for the current analysis. These variables had to meet the further criterion of a response frequency distribution of no greater than an 85%–15% split after dummifying into dichotomous nominal scales. Thus 96 variables were selected for typological analysis.

Data were collected from a saturated sample of 150 metropolitan juvenile courts via telephone interviews.³ The interviews were conducted with two respondents from each court between May 1979 and February 1980. In most cases, a judge and a court executive, such as the director of court services or a chief probation officer, participated in the survey.⁴ The two court representatives, who had been mailed copies of the questionnaire prior to the interviews, each responded separately. The two sets of responses were then compared for agreement to determine the reliability of the data.⁵ To increase reliability, callbacks and site visits were scheduled to resolve discrepancies and a complete response set was constructed for each court. These 150 composite response sets with a resolved 98% agreement between respondents provided the data for analysis.

DATA ANALYSIS

Phase I: Determining the Structure of the Data

In the first phase of the typological analysis, the underlying structure of the data was explored using both principal components and classical factor analyses (Rummel 1970, pp. 104–13). Examination of the rotated solutions (with the number of factors varying between three and 27; varimax criterion [Rummel 1970, pp. 391–93]) yielded eight stable and interpretable factors.⁶ These factors were nearly identical in the principal components

³ In all, 160 courts were identified as meeting the criterion of serving populations of 250,000 or more. Some jurisdictions were subsequently dropped or consolidated, such as the boroughs of New York City and Connecticut courts. Only one court declined to participate.

⁴ Of the responses, 24 are composites from conference calls, from collaboration between respondents prior to the interview, or from single respondents who were judged sufficiently informed to provide full and accurate information. These responses were compared with those from two-respondent courts and found not to differ substantially.

⁵ Initial reliability ranged from .36 to .97, with a mean of .54 and a standard deviation of .123.

⁶ Decisional criteria included the "eigenvalue-one" rule and interpretability (Rummel 1970, pp. 362–64).

and classical factor analyses. The factors loaded on 43 of the 96 variables and accounted for 65.1% of the variance of the reduced data set (that remaining after elimination of nonloading variables). Three factors were eliminated from further analysis because of coding problems and internal inconsistency among related items. In this case, factor analysis represented a good deal more than a data reduction technique. It was an effort to locate the constellations of court characteristics that represent structural dimensions of juvenile justice. It was also an attempt to identify the constructs that court respondents used in defining the structures and procedures in handling juvenile justice case loads. Table 1 shows the five factors that emerged from this analysis. Each factor is regarded as representing a dimension of juvenile court structure.

Factor 1: Status offender orientation/scope of jurisdiction.—The first factor, status offender orientation/scope of jurisdiction, contains a cluster of items relating entirely to the processing of status offenders (youths who have committed acts that would not be offenses if committed by an adult). It captures the basic components of status offender jurisdiction: intake discretion to refer, counsel, or release from detention; the use of nonjudicial conferences to adjust the case; notification of rights if a judicial hearing is to be held; and disposition options available after formal adjudication. The negative loading of the item representing diversion before official court handling is consistent with the interpretation of a status orientation; that is, the court has both the capacity and the willingness to deal with such offenses.

Factor 2: Centralization of authority.—The second factor, centralization of authority, relates primarily to the court administrative control over probation, detention services, and court responsibility for restitution programs. Centralized authority is enhanced through the control and distribution of organizational rewards—for example, hiring and firing, promotions, and incentive rewards. These are managed by the judge and his or her administrative officer or officers. Although authority may be delegated, as in a classical bureaucracy, it is likely to be exercised on a personal basis, not through incumbency in an "office" requiring technical expertise (Weber 1958, p. 295). Persons in probation or detention positions are not likely to be protected by a regular system of appointment and promotion on the basis of a freely negotiated contract or guaranteed fixed salaries for specific duties (e.g., through a union or merit system) (Weber 1958, pp. 196–97).

Factor 3: Formalization.—Factor 3 represents the dimension of formalization. This factor consists of three items directly interpretable as the separation of the adjudication and disposition hearings in formal court procedures. Not only is this dimension descriptive of structural formality, it also provides insight into the use of information at the adjudicatory hearing. The use of social reports as an aid in establishing the jurisdictional

predicate has been a traditional practice of juvenile justice. It is most evident in proceedings in which the demarcation between adjudication and disposition is either nonexistent or, at best, difficult to distinguish (President's Commission on Law Enforcement 1967; IJA/ABA Standards 1977a, pp. 66-67). Although the practice has been defended on the grounds that

TABLE 1
FACTORS OF COURT CHARACTERISTICS

Loading*	Factor 1 (Status Offender Orientation)
.81	Intake or probation officer can refer status offenders to voluntary agency
.77	Intake or probation officer can release status offenders from detention
.74	Judicial disposition option—status offenders can be adjusted and released
.73	Intake or probation officer can counsel and reprimand status offenders
.72	Status offenders are notified of right to counsel at first appearance before a judicial officer
.70	Status offender cases can be disposed through continuance pending adjustment
.64	Use of a nonjudicial conference to adjust status offender cases before petition is filed
.64	Status offenders can be placed in a nonsecure facility
.61	Status offenders are notified of right to confront and cross-examine witnesses at first appearance before a judicial officer
.60	Status offenders are notified of right to silence at first appearance before a judicial officer
-.49	Most status offenders are diverted before official court hearing
Factor 2 (Centralization of Authority on Probation and Detention)	
.74	Court or judge administers probation department
.78	Court or judge controls hiring and firing of probation personnel
.76	Court or judge administers detention hearings
.76	Court or judge controls hiring and firing of detention personnel
.56	Court is directly responsible for the administration of a restitution program
-.47	There is a union for probation officers
-.40	There is a merit system for detention workers
Factor 3 (Formalization of Procedure)	
.92	There is a mandatory minimum time between adjudication and disposition
.94	The mandatory interval can be waived
.65	The court bifurcates the hearings in practice
Factor 4 (Task Specification/Differentiation)	
-.61	The court is one of general jurisdiction
-.60	The prosecutor participates in the decision to file a formal petition
-.51	Appeals go first to an appellate court
.52	"Someone else" (other than prosecutor, judge, probation officer, or police) organizes the facts of the case in court for status offenders
Factor 5 (Discretion)	
.58	Intake or probation staff arranges informal probation for law violators
.56	Intake or probation staff arranges informal probation for status offenders
.46	Intake or probation staff arranges restitution for law violators

* The loadings are from the factor pattern matrix after varimax rotation. All loadings over .40 are displayed.

adjudications, as well as dispositions, must be based on knowledge of all the circumstances pertaining to the case, the issue has been hotly contested on the grounds that such usage of social information interferes with the introduction of relevant evidence and may prejudice the case (Teitelbaum 1967). The formalization of the adjudication process through the bifurcation of hearings has been recommended as a way of limiting this practice (IJA/ABA Standards 1977a).

Factor 4: Differentiation/task specification.—The fourth factor deals with differentiation/task specification. Traditional juvenile justice, though specialized in its jurisdiction, has never been thought to be integrated with the rest of the judicial system (IJA/ABA Standards 1977b). In courts of limited jurisdiction, dealing primarily with juvenile and youth-related matters, appeals are relatively infrequent. When they occur, the hearing is often held de novo in a court of higher trial jurisdiction rather than on direct appeal. These elements of court structure correlate with the absence of task specification in juvenile hearings of the traditional mode. The traditional role of the juvenile court judge incorporates the multiple functions of judge, attorney for the defense, and prosecutor (Emerson 1969, pp. 172–215; Stapleton and Teitelbaum 1972, pp. 111–53). The expansion of the role of the prosecutor in juvenile justice marks a significant change from this practice (Rubin 1980), but the undifferentiated type of hearing is still the hallmark of juvenile justice. It is especially evident in status offender hearings with both the lawyer and the prosecutor absent and “someone else” (e.g., the parent, the complainant, or an agency representative) organizing the facts of the case for presentation in court.

Factor 5: Intake discretion.—A fifth factor concerns the dimension we have called intake discretion. It refers principally to the authority of the probation or intake staff to impose informal probation or restitution without a formal judicial hearing. Discretion may be defined as “relief from law” rather than by other connotations of the word—for example, “absence of law” or “opposition to law” (Rosett 1979). The distinguishing characteristic of this dimension is that it is nonjudicial and it is exercised on cases prior to (or instead of) filing a formal petition. It is a practice typically used by courts with a social service agency approach that regard their task as nonpunitive in character. The informal handling of such cases has been regarded by some researchers as being equivalent to a dismissal of the case (e.g., Cohen and Kluegel 1978).

A further characteristic of this dimension is that it marks the first stage of a juvenile justice system “triage” process whereby youths are pre-screened for formal court appearance. The process of informal probation or restitution handled by the probation department without a judicial hearing is an early form of diversion in many juvenile courts.

Phase II: Construction of the Typology

In the second phase of the analysis, a variable was selected from each of the five factors as an indicator of the factor (see table 2). The criteria for selection were a high loading and conceptual clarity. These five variables were entered into an agglomerative hierarchical cluster analysis (Johnson 1967.⁷ The procedure calculates a distance matrix with a euclidean metric and is based on the "maximum method" for clustering, which produces clusters that are optimally homogeneous and compact. There are no clear-cut statistical criteria for selecting the best clustering solution. Given the purpose of developing a parsimonious typology, a solution was sought that maximized homogeneity within clusters and minimized the number of clusters. Use of these criteria led us to the 25-cluster solution. Within these clusters, courts had identical values on the five classifying variables; homogeneity was indicated statistically by the "maximum distance within a cluster" value of 0.0. Of the 25 clusters, 12 contained three or more courts; these 12 incorporated 130 of the 150 courts in the study. The remaining 20 courts, which appeared as individual "clusters" or in clusters of two courts, appeared to reflect random or error variance.⁸ As a result, these courts were treated as "ungrouped" cases or "outliers" in subsequent analyses.

The 12 clusters were entered into a discriminant analysis that served to test the viability of the five variables as indicators of dimensions of variation of juvenile courts (as they operate within the context of related agencies). The analysis also tested the strength of the groupings against a criterion of use of information (i.e., information from the set of 96 variables). The discriminant analysis revealed that 58 variables, not including the five classifying variables, could discriminate among the clusters. The classifica-

TABLE 2
INDICATORS OF FACTOR STRUCTURE OF JUVENILE COURTS

Factor and Indicator	Factor Loading
1. Intake or probation officer can refer status offender to voluntary agency	.81
2. Court or judge administers probation department.....	.74
3. Mandatory interval between adjudication and disposition can be waived*	.94
4. Prosecutor participates in the decision to file a formal petition.....	-.60
5. Intake or probation staff arranges informal probation for law violators	.58

* Indicates existence of mandatory interval.

⁷ Initially, factor scores were created using the complete estimation method and these composite indices entered into a cluster analysis. It became apparent that the factor scores (again, as complete estimates) contained information that was too complex to allow identification of interpretable clusters (or "types" of juvenile courts).

⁸ Subsequent analyses suggest that these courts may not reflect error variance.

tion power of the discriminating variables was 98.46%. In other words, a classification based on the information provided by the 58 variables would place 98.46% of the courts in the same clusters to which they had been assigned previously. The 12-cluster solution (with 20 ungrouped cases) was selected as the empirical basis for the typology. The cluster analysis produced a monothetic solution (i.e., every court in a cluster has the same value on each variable). (See the Appendix for a list of the courts in each cluster.)

A TYPOLOGY OF JUVENILE COURTS

Table 3 displays the values of the clusters derived from the cluster analysis on each of the dimensions identified through factor analysis of the data.

Clusters 1-4

Clusters 1, 2, 3, and 4 are all characterized by the inclusion of status offenders in their jurisdiction, centralization of authority, and lack of formalization. Cluster 1 is further characterized by low task specification; there is little prosecutorial function. Intake has high discretion to adjust cases informally. Cluster 1 courts may be said to approximate the traditional juvenile court described in the literature. The court is all-inclusive; it is the juvenile justice system. The court maintains control from intake through disposition. Approximately one-fifth ($N = 32$) of the courts in the sample fell in this, the largest cluster.

TABLE 3
AN EMPIRICAL TYPOLOGY OF METROPOLITAN JUVENILE COURTS

CLUSTER (N)	STRUCTURAL DIMENSIONS				
	Scope of Jurisdiction	Centralization of Authority	Formalization	Task Specification	Intake Discretion
1 (32).....	Inclusive	High	Low	Low	High
2 (16).....	Inclusive	High	Low	High	High
3 (7).....	Inclusive	High	Low	High	Low
4 (13).....	Inclusive	High	Low	Low	Low
5 (3).....	Exclusive	High	Low	High	High
6 (4).....	Exclusive	High	Low	Low	High
7 (20).....	Inclusive	Low	Low	High	High
8 (14).....	Inclusive	Low	High	High	High
9 (4).....	Exclusive	Low	Low	High	Low
10 (4).....	Exclusive	High	Low	High	Low
11 (4).....	Inclusive	Low	Low	Low	Low
12 (9).....	Inclusive	Low	Low	Low	High

$N = 130$.

Clusters 2 and 3 can be distinguished from cluster 1 in that there is prosecutorial involvement in the decision to file a petition. In cluster 2 this is combined with intake discretion to place delinquents on informal probation, whereas in cluster 3 intake does not have this discretion.

In cluster 4 courts there is neither prosecutorial involvement nor high intake discretion. There is no apparent court or court-related prescreening of complaints before a judicial hearing; petitions are filed directly.

Clusters 5 and 6

Status offense cases are not within the jurisdiction of these courts. Courts in clusters 5 and 6 resemble those in clusters 1–4, however, in being characterized by centralization of authority and low formalization of procedure. In both clusters intake is involved in prescreening of cases, but in cluster 5 the prosecutor is also involved in the decision to file a formal petition.

Clusters 7–9

Clusters 7, 8, and 9 represent a major departure in that the system is decentralized; the court does not administer probation. Furthermore, in these clusters the prosecutor is involved in the decision to file a petition. These characteristics are commonly associated with the criminal justice ideal-typical model. It is a model similar to that described by Eisenstein and Jacob (1977) as an association of work groups who share an ideology but whose day-to-day activities are structured and organized by their participation in differing "sponsoring" organizations (e.g., the judiciary, the prosecutor's office, the probation and parole services, the clerk's office, and the public defender's office). These work groups participate in a "case," but they are bound by loose interorganizational ties rather than by an integrated bureaucratic structure headed by a judge. The judge can maintain authority in the workplace of the courtroom, but his authority is limited in that the judiciary is neither the primary employer nor the exclusive organizer of the other work groups. Insofar as the ideology of due process is concerned with the act, not the condition, we expect to find more formal procedures designed to limit information used in adjudication to that which is legally relevant. One result is to reserve the probative value of offender (social) information for a separate disposition hearing. The 14 courts in cluster 8 are the only ones included in the typology that require bifurcation of the adjudicatory hearing.

Clusters 7 and 8 are further distinguished by a double prescreening process; the noncourt probation or intake has the discretion to handle a case informally, and the prosecutor must make the final decision to file. We

expect that such a system will define more precisely the types of cases going to a judicial hearing and, although the total system (represented by the court plus an independent probation department) may be status offender oriented, these cases are not likely to appear in court.

Cluster 9 is the closest to a felony justice model in that along with decentralization of authority and prosecutorial involvement in intake these courts have no jurisdiction over status offenders. We note that only four courts fall in this cluster (table 3).

Cluster 10

The four courts in cluster 10 are similar to cluster 9 courts on all but one dimension. The court maintains control of probation, even though the prosecutor is involved in prescreening of cases.

Clusters 11 and 12

Courts in clusters 11 and 12 have separated probation from the executive control of the court. They do not, however, share the screening function with a prosecutor. Notably, eight of the 13 courts in these two clusters are in New York State. This appears to reflect a historical artifact. In 1975 the New York Court of Appeals "held that legislative transfer of probation services from judicial to executive responsibility is constitutionally permissible" (IJA/ABA Standards 1977*b*, p. 17). These courts also include status offenders within their jurisdiction and do not require the bifurcation of the adjudicatory and disposition hearings.

DISCUSSION AND IMPLICATIONS

In part, the empirical typology of metropolitan juvenile courts reflects the existence of the two major types of juvenile courts ("traditional" and "due process") suggested in the literature. More important, however, it reveals variations in court structure and procedure that are not captured adequately by existing simplistic typologies.

The variations described may reflect changes in juvenile court structure. Although the present survey can provide only a static portrait, the typology does suggest the nature of change and some of its directions. We conceive of juvenile courts as "open systems" (Katz and Kahn 1966) reacting to exogenous events and adapting to strain through the gradual introduction of new elements. For example, the *Gault* case (387 U.S. 1 [1967]) mandates the introduction of defense counsel but defines neither its precise role

nor the stage at which counsel is to be assigned. Studies of the role of attorneys in juvenile court (Stapleton and Teitelbaum 1972; Clarke and Koch 1980) suggest considerable role conflict when adversary-oriented counsel are introduced without adapting other elements of the system to a conflict model of adjudication. The introduction of a more active prosecutorial role (as in clusters 2 and 3) may be an adaptive mechanism that reduces the role strain of a judge who had acted as both prosecutor and judge prior to the extensive use of defense counsel.

Similarly, a triage prescreening system (as in clusters 2, 3, 5, and 10) to determine which cases become formal may be an adaptation to *Gault* and the diversion movement. The triage identifies cases that are not likely to result in incarceration and, therefore, do not require full application of due process guarantees. This adaptive strategy, then, allows the management of the introduction of new or changed roles into the structure and procedures of the court.

The typology reinforces Hagen's concept of juvenile justice as a loosely coupled set of subsystems (Hagen 1979; Gove 1980). There are several implications. If juvenile courts are not represented by a single, uniform system of case processing, it follows that research will have to take the variation into account and sample accordingly. Present studies of case decisions in juvenile justice may reflect sampling errors and system differences. Cluster 4 courts, for instance, represent neither probation nor prosecutorial screening of cases for formal petition, whereas clusters 2, 3, 5, and 10 represent double screening and clusters 7, 8, and 9 are separately institutionalized juvenile justice processing systems. As simple a procedure as taking all court cases as the sampling frame will yield differing populations (for cross-court comparisons) depending on the system or systems selected. Because so few studies have used multiple-court comparisons, this basic methodological point is largely overlooked.

Past studies cannot be used to test hypotheses from the current research and typology. These studies (and the present one) are time bound and static. Nevertheless, we suggest that some of the contradictions in the research of the past decade can be understood against a background of structural analysis of case processing. That suspicion is partly confirmed by research conducted by two of us and a third person (Stapleton and Smith 1980; Ito and Stapleton 1981). Case outcomes and the determinants of decision making in juvenile justice should not be interpreted without knowledge of structure and procedure. A simple checklist of "components" of justice is not enough. An adequate understanding of juvenile courts and of the processes and criteria of decision making in juvenile cases requires careful treatment of the variations in juvenile court structures and procedures, as these exist in the context of related justice agencies.

APPENDIX

Courts Identified by Cluster Number

Cluster 1

Birmingham, Ala.
 Mobile, Ala.
 Decatur, Ga.
 Ft. Wayne, Ind.
 Gary, Ind.
 Baton Rouge, La.
 Boston, Mass.
 Cambridge, Mass.
 Fall River, Mass.
 Worcester, Mass.
 Detroit, Mich.
 Flint, Mich.
 Lansing, Mich.
 Pontiac, Mich.
 Clayton, Mo.
 Independence, Mo.
 St. Louis, Mo.
 Hackensack, N.J.
 Jersey City, N.J.
 Toms River, N.J.
 Trenton, N.J.
 Akron, Ohio
 Columbus, Ohio
 Tulsa, Okla.⁹
 Erie, Pa.
 Greensburg, Pa.
 Norristown, Pa.
 Wilkes-Barre, Pa.
 Providence, R.I.
 Chattanooga, Tenn.
 Knoxville, Tenn.
 Memphis, Tenn.

Cluster 2

Tucson, Ariz.
 San Jose, Calif.

Denver, Colo.
 Hartford, Conn.
 Geneva, Ill.
 Joliet, Ill.
 Wheaton, Ill.
 Minneapolis, Minn.
 Omaha, Nebr.
 Morristown, N.J.
 Albuquerque, N.Mex.
 Las Vegas, Nev.
 Youngstown, Ohio
 Portland, Oreg.
 Austin, Tex.
 San Antonio, Tex.

Cluster 3

Little Rock, Ark.
 Phoenix, Ariz.
 Gretna, La.
 New Orleans, La.
 Mt. Holly, N.J.
 Elyria, Ohio
 El Paso, Tex.

Cluster 4

Honolulu, Hawaii
 Indianapolis, Ind.
 Springfield, Mass.
 Grand Rapids, Mich.
 Greensboro, N.C.
 New Brunswick, N.J.
 Newark, N.J.
 Paterson, N.J.
 Canton, Ohio
 Dayton, Ohio
 Toledo, Ohio
 Philadelphia, Pa.
 Nashville, Tenn.

⁹ The prosecutor is involved in the decision to file in certain cases in Tulsa. Variable 195 was therefore scored in the "other" category and recorded as zero when the variables were dummied (i.e., it is treated as error variance).

Cluster 5

Wilmington, Del.
Media, Pa.
West Chester, Pa.

Cluster 6

Allentown, Pa.
Pittsburgh, Pa.
Reading, Pa.
York, Pa.

Cluster 7

Los Angeles, Calif.
Martinez, Calif.
Redwood City, Calif.
Riverside, Calif.
Sacramento, Calif.
San Bernardino, Calif.
San Diego, Calif.
Santa Barbara, Calif.
Ventura, Calif.
Bartow, Fla.
Clearwater, Fla.
Jacksonville, Fla.
Miami, Fla.
West Palm Beach, Fla.
Chicago, Ill.¹⁰
Baltimore, Md.
Towson, Md.
Syracuse, N.Y.
Charleston, S.C.
Dallas, Tex.

Cluster 8

Bakersfield, Calif.
Ft. Lauderdale, Calif.
Oakland, Calif.
Salinas, Calif.

San Francisco, Calif.
Santa Ana, Calif.
Stockton, Calif.
Orlando, Fla.
Tampa, Fla.
Annapolis, Md.
Rockville, Md.
Upper Marlboro, Md.
Madison, Wis.
Waukesha, Wis.

Cluster 9

Golden, Colo.¹¹
Greenville, S.C.¹²
Ft. Worth, Tex.
Seattle, Wash.

Cluster 10

Washington, D.C.
Cincinnati, Ohio
Everett, Wash.
Spokane, Wash.

Cluster 11

Buffalo, N.Y.
Charlotte, N.C.
Raleigh, N.C.
Fairfax, Va.

Cluster 12

Camden, N.J.
Albany, N.Y.
Mineola, N.Y.
New City, N.Y.
Riverhead, N.Y.
St. George, N.Y.
Utica, N.Y.
White Plains, N.Y.
Norfolk, Va.

¹⁰ The cluster solution grouped this court in this cluster because of measurement error.

¹¹ See n. 10 above.

¹² Ibid.

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Capital Cities in the American Urban System: The Impact of State Expansion¹

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In recent decades, U.S. federal and state budgets have expanded faster than the gross national product. This expansion has increased the growth of state and national capital cities. Empirical evidence shows that capitals have grown more than other cities in terms of population, income, transportation and communication, and occupations in the more advanced sectors of the economy.

During this century, federal and state government budgets have increased in the United States, even relative to a growing national economy. The authority of these organizations has expanded along with their resources. Despite the magnitude of this expansion, the spatial implications of state growth have been relatively ignored by American urban sociology.²

This neglect is unwarranted, given the increased societal importance of the state. State expansion, through the development of capital cities, exerts a major influence on the spatial distribution of resources. In this research note, we examine the effects of state expansion on the structure of the American urban system, showing that political expansion has awarded developmental advantages to capital cities. We assess the effect of capital city status on population growth, income, transportation and communication resources, and occupational structure. Because state capitals have been regarded historically as backward areas, growth patterns favoring capital cities signal a potential restructuring of the intercity hierarchy.

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² For exceptions, see Sjoberg (1963), Friedmann (1972), Berry (1973), and Stone (1972). The rise of preindustrial cities is thought to be closely associated with state or empire formation and the corresponding dominance of the hinterland (Sjoberg 1960). And histories of European city development often stress political factors: for instance, Hawley (1971) emphasizes the importance of political capitals in the 17th-century development of modern urbanization. Other investigators have discussed similar phenomena in modern Africa (Mabongunji 1973; Hamden 1964), the Soviet Union (Huzinec 1978), and the modernizing world (Friedmann 1973; Friedmann and Wulff, 1976).

GOVERNMENTAL EXPANSION IN THE UNITED STATES

Consider the expansion of the state structures in the United States during this century. Figure 1 presents a historical overview of governmental finances from 1902 to 1977. The top line of the figure shows total governmental expenditures at all levels as a proportion of the gross national product (GNP). The other lines show similar ratios for federal expenditures and for state and local revenues.³

The overall trends in the budgetary data are easily summarized. All three levels of government were relatively unimportant parts of the national economy in the first decades of this century. All three have since expanded greatly, even relative to the rapidly growing economy. Massive federal expansion was associated with World War II and the reorganization for the Cold War afterward. The federal government has not expanded relative to the economy in recent decades. In contrast, the state and local levels have expanded steadily throughout the century, but especially in the recent era. During this period, intergovernmental funding has also increased rapidly—to the point where over one-fourth of state revenue now comes from the federal government and about two-fifths of local governmental revenue comes from the higher levels (U.S. Bureau of the Census 1974, 1978).

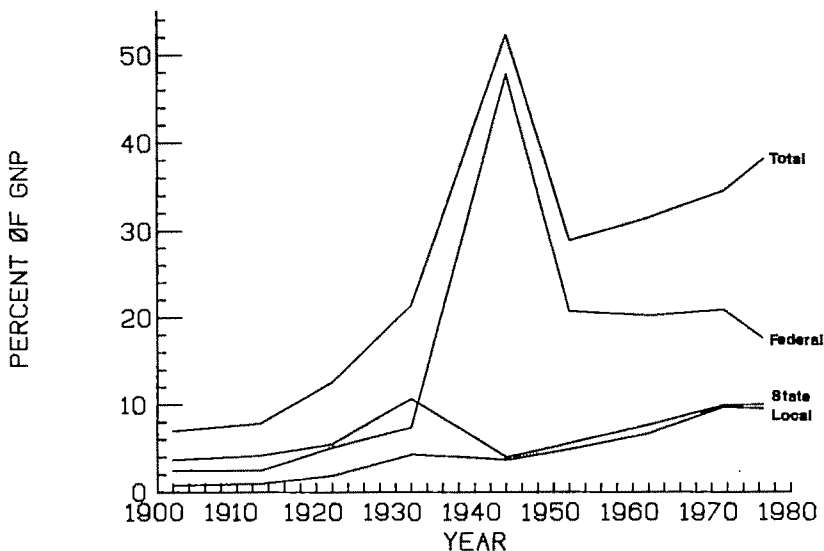


FIG. 1.—U.S. government finances. Data sources: U.S. Bureau of the Census 1974, 1978.

³ For the state and local governments, we use revenue rather than expenditure data in order to avoid fungibility problems in ascertaining the extent of dependence on federal funds. This decision makes no substantive difference in the trends we discuss. As in the practice of national accountants, funds may be included more than once as they pass through the different levels of government.

The general trend toward expansion at the state level is especially important to our argument because most of our findings concern the growth of state capitals. This trend may continue. First, the "New Federalism," which through revenue sharing has brought both money and decision-making power to the state level, is likely to continue. Second, the increased state dependence on federal funds brings with it pressure for bureaucratic isomorphism with the more rapidly developing federal funding structure (Elazar 1974). Third, state governments have expanded significantly the scope of their functional authority and responsibilities, as well as their tax bases (Mandeleker and Netsch 1977). Finally, states have increased their control over local government spending through both mandate requirements⁴ and revenue controls (the latter have been strengthened by popular resistance to local property taxes [U.S. Advisory Committee on Intergovernmental Relations 1978]).

These trends in governmental expansion should have a direct spatial manifestation. Following periods of political expansion, we expect increased growth at the center of political power—the capital. Thus we expect exceptionally rapid growth and development in the national capital during and immediately after World War II—the period of greatest federal expansion. State capitals should grow disproportionately more during more recent decades—especially after 1960 (during the war itself state budgets *declined* relative to the GNP). These expectations are not temporally rigid; indeed, the redistribution of resources in an urban system is a massive process that requires many years to reach equilibrium. Thus, we expect a significant time lag between the periods of state expansion and the date of rapid capital city growth.

A substantial proportion of political expansion affects capital cities. Even if state funds are broadly dispersed from their point of allocation, bureaucratic administrative structures are usually found in capitals. Thus, state expansion is very likely to increase governmental employment in capital cities. In addition, many private organizations and lower-level governmental bodies recognize incentives for location in the capital city: they open branch or liaison offices in the capital or move corporate and administrative headquarters to the capital. States, for instance, now routinely maintain offices in the national capital, as cities do in state capitals. This practice is increasingly customary with professional and voluntary associations and with business firms.

Each of these private-sector activities is tied to state expansion, which is occurring rapidly. Our first hypothesis is that during periods of state growth, the populations of capital cities grow more rapidly than those of

⁴ In 1976, 50% of all functional components in local governments were subject to state mandates (U.S. Advisory Committee on Intergovernmental Relations 1978). These mandates, along with the financial weakness of local governments, mean that future growth in local services will require additional supporting state personnel.

comparable cities. This growth is the direct consequence of increased employment by the government, increasing numbers of persons and groups interacting with the government, and increased demands by these new residents for services.

Second, we expect incomes to grow faster in capital cities than in comparable cities during periods of political expansion. This results in part from the increased economic activity and opportunities arising from rapid population growth. It results also from the increasing power of the governmental bureaucracies and the rise of ideologies depicting governmental functions as increasingly important to society. Heightened bureaucratic incomes increase the average income of the capital city, but so do the incomes of the new migrants to the capital, who are likely to be high-income managers, executives, and professionals.

Third, we expect capital cities to obtain disproportionately more "growth-inducing" resources, such as transportation and communication facilities, because the state itself tends to control or regulate these resources.

Finally, we expect the most advanced sectors of the economy and occupational structure to expand disproportionately in capital cities. This expansion is due in part to the economic advantages of location in growing and rich cities. Also, those sectors naturally linked to bureaucratic state authority grow most rapidly. That is, sectors most densely linked to complex aspects of economic life (e.g., the tertiary sector generally) come under state control or regulation and obtain the greatest advantages from capital city location.

EMPIRICAL ANALYSES

Population

We expect the populations of capital cities to grow more rapidly than those of others, following periods of political expansion. Although we expect the rate of growth in capital cities to be independent of city size, the absolute number of people gained should depend on initial city size. These considerations lead us to formulate interactive models in which we test for the effects of capital city status as it interacts with initial city population.

Our sample is the complete set of Standard Metropolitan Statistical Areas (SMSAs) in 1970. This set includes the national capital, 35 state capitals, and 208 other urban areas.⁶ Population data are collected for

⁶ Ideally, our sample would contain all urban areas, including the 50 state capitals. We decided to restrict the analysis to SMSAs primarily for reasons of data availability. However, we think this decision is justified on substantive grounds because the smaller urban areas are insignificant with respect to the intercity urban hierarchy. We do not think the restriction introduces any bias in our analysis.

each SMSA in 1950, 1960, 1970, and 1975 from the standard sources (given in table 1). For earlier years, data are taken from a special Census Bureau aggregation for the counties included in each SMSA in 1960 (see U.S. Bureau of the Census 1962). In this way, population figures for the same areas for 1910, 1920, 1930, and 1940 are obtained.

The effects of capital city status are estimated by a linear panel regression model predicting population size 10 years later. Capital city dummy variables, interacted with population, are included to test our central hypothesis.⁶

Table 1 presents the results of the weighted least squares estimates of the panel model. The coefficients for the national capital show that it grew less rapidly than other cities in the decades before 1930. Its growth from 1930 to 1950 was extraordinary, reflecting governmental expansion from the New Deal and World War II. From 1950 to 1970, the capital grew a little more than other cities.

State capitals also tended to grow less rapidly (but not significantly so) than other cities until 1930. But since 1930, the state capitals have grown more rapidly than comparable cities (except for the 1960–70 interim); for the 1930s and early 1970s, the associated coefficients are statistically significant.

These results demonstrate that capital cities have grown disproportionately, but their growth may be due to factors other than state expansion. In particular, we explore whether capital city status has an effect beyond that expected from industrial mix, state population size, and regional movements. For this purpose, we present table 2, which contains estimates of the model with these control variables over the period of greatest state level growth: 1960–75.

Again the results show considerable support for our general hypothesis. The state capital coefficient is positive in all equations and significant in all but one. These findings are important because they demonstrate that capital city growth is not a spurious consequence of region, industrial mix, or state population—all three major factors in the national population shifts occurring during this period.

Income

We expect capital city status to have positive effects on income growth in recent periods. Income data are taken for the complete set of SMSAs for 1949, 1959, 1969, and 1974. For the three earlier points, we employ median family income. This variable is not available for 1974, so we use per capita

⁶ Early analysis displayed heteroscedasticity of the residuals and thus violated a critical assumption of ordinary least squares estimates. Consequently, we estimated the model with weighted least squares, using the lagged population variable as a weight. This remedy proved adequate.

TABLE 1
WEIGHTED LEAST SQUARES ESTIMATES OF THE EFFECTS OF CAPITAL CITY STATUS ON POPULATION SIZE

INDEPENDENT VARIABLES	DEPENDENT VARIABLE: POPULATION									
	1910	1920	1930	1940	1950	1960	1970	1975		
Intercept.....	6.16**	.395	3.82	9.02**	16.2**	-32.6	-175.	45.6*		
Population 1900.....	1.32**									
Population 1910.....		1.31**								
Population 1920.....			1.28**							
Population 1930.....				1.03**						
Population 1940.....					1.15**					
Population 1950.....						1.35**				
Population 1960.....							1.32**			
Population 1970.....								1.07**		
State capital X population ^a	-.087	-.055	-.021	.052**	.052	.088*	-.048	.098**		
National capital X population ^b	-.165	-.022	-.108	.397**	.350*	.020	.116	-.018		
N.....	210	212	212	212	212	172	224	238		

SOURCE.—U.S. Bureau of the Census (1956, 1962, 1967, 1972, 1977).

NOTE.—Population variables are in hundreds.

^a Multiplicative interaction variable which takes the value of the lagged population variable for state capitals and zero for other SMSAs.

^b Multiplicative interaction variable which takes the value of the lagged population variable for the nation capital and zero for other SMSAs.

* $P < .10$.

** $P < .01$.

TABLE 2
WEIGHTED LEAST SQUARES ESTIMATES OF THE EFFECTS OF CAPITAL CITY STATUS ON POPULATION SIZE IN 1975

INDEPENDENT VARIABLES	DEPENDENT VARIABLE: POPULATION IN 1975					
	Equation 1	Equation 2	Equation 3	Equation 4	Equation 5	Equation 6
Intercept.....	-421.	2,790.	1,428.	117.	-2,780.	2,590.
Population 1960.....	140.***	116.***	135.***	134.***	125.***	102.***
Manufacturing employees 1961.....	-114.**	...	-152.***	-160.**	...	41.1
Service employees 1961.....	...	153.	109.	142.	...	322.**
State population 1961.....	1.03
Region dummies:						
South.....	23.5***	17.7***
West.....	19.7***	16.0**
Southwest.....	8.67	24.6***
Northeast.....	-10.3	-11.8**
State capital X population 1960.....	8.93	15.1***	10.8**	10.8**	11.2*	11.8**
National capital X population 1960.....	13.3	24.6	10.9	10.4	36.5	38.8*
N.....	126	120	120	120	120	120

Sources.—U.S. Bureau of the Census (1967); U.S. Department of Labor, Bureau of Labor Statistics 1962, 1978).

Note.—Population variables are in hundreds. Population 1960 is used as the weighting variable in all equations.

*. $P < .10$.

** $P < .05$.

*** $P < .01$.

income instead (and as a 1969 control variable). Again, the hypothesis is tested by estimating linear panel models with a lagged dependent variable, dummy variables for national and state capitals, and population as a control variable.

Table 3 shows the ordinary least-squares estimates of the effect of capital city status on income growth over time. In each equation estimated, the effects of capital city status are positive and large. They are not always significant, but the results seem quite regular. Across each time period from 1949 to 1974, both state and national capitals grow more in income than do other cities. Our hypothesis receives, therefore, substantial support.

TRANSPORTATION AND COMMUNICATION RESOURCES

A central part of our argument is that capital cities grow disproportionately because the decision makers of the state control many of the traditional growth-inducing resources. Many of these resources are the capital-intensive infrastructure required to support large transportation and communication networks. We contend that the allocation of these resources will favor the political capitals, reflecting the ideological commitment to the importance of the state as well as community self-interest. In both cases, the net result of such investments is the stimulation of current and future growth in capital cities.

The empirical demonstration of this hypothesis relies mainly on transportation data; communication resource variables are more difficult to obtain. Specifically, we examine in depth the automobile and air transportation systems. For the automobile system, we explore the effects of capital city status on the U.S. interstate highway system. The number of extra-urban connections to the interstate system and the total interstate highway mileage within the urban area are examined. We also investigate the total metropolitan area mileage of arterials, freeways, and highways. The air transportation system analysis considers two dependent variables: number of air passengers enplaned and number of aircraft departures. And, finally, the one communication resource variable we analyze is volume of U.S. priority mail (in tons). This variable gives a crude indication of the magnitude of communication flows into and out of an urban area, which has been regarded by ecologists as a growth stimulus (Duncan 1964).

Table 4 presents the analysis for the transportation and communication variables.⁷ The state capital effect is significant and positive for both

⁷ In the automobile transportation system analysis, we estimate cross-sectional rather than panel equations, mainly because we were unable to locate historical data. We argue that in this instance the cross-sectional strategy is valid because (1) the substantive variables are investment (or stock) variables and not growth (or flow) variables, and (2) since infrastructural variables change slowly over time, the corresponding coefficients of a panel equation would be very similar.

TABLE 3
ORDINARY LEAST-SQUARES ESTIMATES OF THE EFFECTS OF CAPITAL CITY STATUS ON INCOME

INDEPENDENT VARIABLES	DEPENDENT VARIABLE			
	Mean Family Income			Per Capita Income 1974
	1959	1969	1969	
Intercept.....	603.	1,928.	1,194.	721.
Per capita income 1969.....	1.24***
Mean family income 1959.....	1.47***
Mean family income 1949.....	1.60***	2.40***
Population*.....	.003	.010***	.006**	-.001
State capital.....	49.98	249.6**	161.7*	33.4
National capital.....	112.3	638.7	471.5	457.4***
N.....	171	170	223	238

SOURCES.—U.S. Bureau of the Census (1956, 1962, 1967, 1972, 1977).

NOTE.—Income variables are in dollars; population is in hundreds.

* Population variable is measured at the same time as the lagged dependent variable.

* $P < .10$.

** $P < .05$.

*** $P < .01$.

TABLE 4

ORDINARY LEAST-SQUARES ESTIMATES OF THE EFFECTS OF CAPITAL CITY STATUS ON TRANSPORTATION AND COMMUNICATION VARIABLES

INDEPENDENT VARIABLES	DEPENDENT VARIABLE									
	Inter- state Connections 1975	Total Miles (1972)			Passengers Enplaned			Aircraft Departures		U.S. Priority Mail 1977
		Inter- state	All Arterials	Freeways	Highways	1977	1970	1977	1970	
Intercept.....	1.86	8.16	51.1	3.21	96.0	106,991.	144,707.	-819.	-3,379.	49,772.
Air passengers enplaned 1970.....	1.48***
Air passengers enplaned 1960.....	3.25***
Aircraft departures 1970.....	1.10***
Aircraft departures 1960.....	1.84***	...
U.S. priority mail 1970.....	~1.54***
Population 1975.....
Population 1970.....004***	.064***	.008***	.089***	28.7***	...	-.363***	...	-4.84*
Population 1960.....	-38.7***	...	-.458***	...
State capital.....	1.33***
National capital.....	.457
State capital X population ^a002***	.011	.002**	.015	20.8***	51.6***	.435***	...	42.6***
National capital X population ^b002**	.008	.001	.009	-28.2***	-28.3	-.352*	-3.05***	36.0***
N.....	238	223	223	223	233	122	125	122	125	122

Sources.—Rand McNally (1976); U.S. Department of Transportation (1976); U.S. Department of Transportation, Federal Aviation Administration (1960, 1970); U.S. Civil Aeronautics Board (1977); U.S. Bureau of the Census (1962).

^a Multiplicative interaction variable that takes the value of the lagged population variable for state capitals and zero for other SMSAs.

^b Multiplicative interaction variable that takes the value of the lagged population variable for the national capital and zero for other SMSAs.

* $P < .10$.

** $P < .05$.

*** $P < .01$.

interstate system variables. It is positive but fails the significance tests for the arterials and highways equations.¹ For each of the other equations, however, it is significant and positive. Thus, state capitals appear to have a disproportionate share of the road systems, air traffic, and mail volume. The national capital variable does not behave as predicted, but that is not surprising, because we do not analyze data for the period before 1960.

OCCUPATIONAL STRUCTURE

We have argued that the occupational structures of capital cities will represent parts of the advanced sectors of the economy more than will those of noncapitals. The empirical analyses employ the Bureau of Labor Statistics periodic employee survey of metropolitan areas. Again, the SMSA is used as the unit of analysis. We collected data for each of the following classifications within the nonagricultural labor force: manufacturing employees; construction employees; mining employees; finance, insurance and real estate employees; transit and public utilities employees; wholesale and retail trade employees; services employees; and government employees. Finance and services employees represent occupations in the advanced tertiary sector of the economy. However, we also expect capital cities to have more employees in the governmental, transit and public utilities, and construction classifications. The first two are obvious areas of governmental effects, while the last is the consequence of increased growth and investment patterns.

Descriptive statistics illustrate that capital cities have considerably different occupational structures. In 1978, the national capital had only 3.6% of its nonagricultural labor force engaged in manufacturing, while the mean was 18.6% for state capitals and 22.7% for noncapitals. In the same year, the national capital had the highest proportions of the labor force in construction (5.1%), services (23.9%), and government (37.8%). State capitals had the next highest percentages in these occupations with 4.5% of all employees in construction, 19.3% in services, and 21.1% in government. The noncapital cities had 4.1% of the labor force in construction occupations, 18.3% in services, and 15.6% in government. These findings agree with our expectations.

Table 5 presents weighted least-squares estimates for capital city status effects on each occupational classification. Again, we use a panel model with a population control variable and population-interactive capital dummies. As the table shows, state capital city status exhibits positive and significant effects for employees in construction, transit and public utilities, wholesale and retail trade, finance, insurance and real estate, services and government. The coefficients of the capital city variables for mining and manufacturing employment are not significant.

TABLE 5
WEIGHTED LEAST-SQUARES ESTIMATES OF THE EFFECTS OF CAPITAL CITY STATUS ON
CHANGES IN LABOR FORCE DISTRIBUTION 1960-78

INDEPENDENT VARIABLES	DEPENDENT VARIABLES: 1978 EMPLOYEES							
	Total Non- agricul- tural	Mining	Con- struction	Manufac- turing	Transit and Public Utilities	Wholesale and Retail Trade	Finance, Insurance, and Real Estate	Service Government
Intercept.....	63.6**	5.58	8.34***	7.06	3.30	22.2***	7.78***	11.5* 13.6**
Total nonagricultural employees 1961.....	.429**	1.19***	1.59***	.803***	1.04***	1.58***	1.55***	2.08***
Mining employees 1961.....								
Construction employees 1961.....								
Manufacturing employees 1961.....								
Transit and public utilities employees 1961.....								
Wholesale and retail trade employees 1961.....								
Finance, insurance, and real estate employees 1961.....								
Service employees 1961.....								
Government employees 1961.....								
Population 1960.....	.329***	-.007	-.007***	.042***	.002	-.002***	.0002***	.001*** 1.47***
State capital X population 1960 ^a131***	.0002	.004***	.005	.005***	.021***	.007***	.015*** .018
National capital X population 1960 ^b202**		.003	-.032	.006	.018	.008	.026 .031
N.....	122	46	123	128	123	122	123	122 123

SOURCES.—U.S. Department of Labor, Bureau of Labor Statistics (1962, 1978); U.S. Bureau of the Census (1967).

NOTE.—Employee variables are in thousands; population is in hundreds.

^a Multiplicative interaction variable that takes the place of the lagged population variable for state capitals and zero for other SMSAs.

^b Multiplicative interaction variable that takes the value of the lagged population variable for the national capital and zero for other SMSAs.

* $P < .10$.

** $P < .05$.

*** $P < .01$.

The results for the national capital parallel those for the state capitals, showing substantial positive effects for the services and government employees sectors. The capital, however, grew less rapidly than other cities in manufacturing employees.

Overall, the estimates conform to our predictions: capital cities have disproportionate numbers of employees in the advanced and service sectors of the economy.

DISCUSSION

The empirical analyses above demonstrate the effects of capital city status on a range of variables. The observed positive effects of capital city status, of course, run against the grain of American political history: for geographical reasons the national capital was located in an out-of-the-way place with few natural or exchange advantages and for more than a century was a national joke. State capitals, too, were often located in places far from naturally advantageous and were often conspicuous for their lack of growth and development. In our analyses, we did not include control variables capturing the typical natural and exchange disadvantages of U.S. capitals—the reader must simply bear in mind that they generally had more such disadvantages than most other cities, and the history of their development shows it.

The processes of change and growth discussed here operate slowly, but over long periods of time. The sociological literature assumes that patterns of urban dominance are relatively fixed, as the exchange factors creating them continue to operate and are reinforced over time. This is certainly true, in that city size and resource distributions are highly correlated over time. But certain major structural changes, such as the rising importance of the state system, introduce new factors and slowly change the prevailing pattern. Our analysis demonstrates an instance of this phenomenon. Moreover, we have shown that many of the growth-inducing resources which the state controls should ensure and increase the growth of capital cities. Thus, we are describing the beginning of a process that might result in significant restructuring of the urban hierarchy when it attains equilibrium. The effect of transportation system development will be felt for many years: only the future will tell in what magnitude.

In conclusion, note that the state expansion process we have described occurred first and with greatest impact in the federal government. Consequently, the tremendous growth of Washington, D.C., does not seem surprising. It does, however, point to the comparative implications of this study. In particular, we suggest that as worldwide state expansion occurs, so too will rapid capital city growth. In addition, we expect this impact to be most pronounced in countries that enter the world market without significant industrial development internally (e.g., 19th-century Europe,

and Africa today): in other words, countries in which the state organized the economy initially. These issues, however, are open to future comparative research.

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On Durkheim's Explanation of Division of Labor¹

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Durkheim's explanation of the division of labor is shown to be faulty in several major respects. While his metatheoretical critique of utilitarian social theory, which was closely intertwined with his analysis of the division of labor, is still persuasive, his causal explanation of the division of labor is questionable wherever it modifies the earlier body of thought. Ironically, it was his metatheoretical concerns expressed in the critique of utilitarian social theory that flawed his contributions to a causal explanation of social differentiation.

In *De la division du travail social*, Émile Durkheim offered a causal explanation for secular increases in the division of labor and the differentiation of social structure. This explanation is severely flawed; in fact, it fell behind the advances in understanding reached by earlier writers, including Karl Marx in *Capital* (see Marx [1867] 1959, chap. 14). A critical review of the arguments Durkheim advanced nearly 90 years ago is of more than mere historical interest, because the work has become enshrined as a classic. This is due largely to its forceful criticism of utilitarian social theory, criticism which, together with similar attacks and revisions, established the distinctive perspective of 20th-century sociology.² Durkheim's discussion of

¹ This essay grew out of a chance discovery. The German journal *Sociologische Revue* had asked me to write a review of the first German edition of Durkheim's work, which was published only in 1977. When I reread the book and some of the secondary literature with a special focus on the causal explanation offered—a subject relevant to my own current work—I realized that certain of Durkheim's arguments were flawed in ways that had been overlooked until now. I wish to thank Robert K. Merton for encouraging me to explain my critical observations in more detail than was possible in that review. After all, writing an essay on what is wrong with a 19th-century analysis of the division of labor is carrying the division of labor far indeed; it does need encouragement. I wish also to thank Mark Shields and Kurt Wolff for their comments on an earlier draft of this paper and Alessandro Pizzorno for a stimulating discussion of the subject. Requests for reprints should be sent to Dietrich Rueschemeyer, Department of Sociology, Brown University, Providence, Rhode Island 02912.

² See Parsons (1937). While Parsons leaves Marx out of his review of major social theorists of the 19th century, Marx's comments on classical economics and his theoretical orientation in general can be read as a critique of utilitarian social theory which anticipates and complements that of Durkheim. These critiques remain crucially important even though the utilitarian theory criticized is not, as Camic (1979) has shown convincingly, identical with the ideas of David Hume, Adam Smith, or John Stuart Mill.

the division of labor itself, aside from its various links to the critique of utilitarian ideas, is of lesser stature and would have been much less enduring. Yet, because of Durkheim's role as a founder of modern sociology, his explanation has been claimed to be the forerunner of contemporary ecosystem theory, and major arguments of his analysis have been readopted in that theoretical tradition (Schnore 1958).

Durkheim's explanation begins with a formulation of the problem that is as global as possible and thus hinders, if it does not preclude, empirical exploration and testing: "If one takes away the various forms the division of labor assumes according to conditions of time and place, there remains the fact that it advances regularly in history. This fact certainly depends upon equally constant causes which we are going to seek" (Durkheim [1893] 1964, p. 233).

The fundamental causal factors Durkheim offers in explanation—large population size and density of interaction ("moral" or "dynamic" density)—had been identified by others earlier. Already Adam Smith [1776] 1937, bk. 1, p. 17) had pointed to sufficient demand as a necessary condition for specialization, by stating that "division of labor is limited by the extent of the market"; and Durkheim himself refers to Comte for the idea that density of interaction is the decisive factor. Karl Marx gives the same explanation for division of labor in society, that is, for specialization among autonomous producers, which he distinguishes from division of labor in manufacture and in modern industry:

Just as a certain number of simultaneously employed laborers are the material pre-requisite for division of labor in manufacture, so are the number and density of the population, which here correspond to the agglomeration in one workshop, a necessary condition for the division of labor in society. [Here, a footnote quotes James Mill and Th. Hodgskin who make the same point.] Nevertheless, this density is more or less relative. A relatively thinly populated country, with well-developed means of communication, has a denser population than a more numerous populated country, with badly developed means of communication; and in this sense the Northern States of the American Union, for instance, are more thickly populated than India. [Marx (1867) 1959, pp. 325–53]

Durkheim modifies this inherited explanatory pattern in three ways. First, he insists that differentiations in social structure always presuppose a unity of the social whole within which the division of labor takes place: "The division of labor can . . . be produced only in the midst of a pre-existing society. By that we do not mean to say simply that individuals must adhere materially, but it is still necessary that there be moral links between them" (Durkheim [1893] 1964, pp. 276–77). This insistence is related to his critique of utilitarian theory, especially the version of that theory presented by Herbert Spencer: "If this important truth has been

disregarded by the utilitarians, it is an error rooted in the manner in which they conceive of the genesis of society. They suppose originally isolated and independent individuals, who, consequently, enter into relationships only to cooperate, for they have no other reason to clear the space between them and to associate. But this theory, so widely held, postulates a veritable *creatio ex nihilo*. . . . Collective life is not born from individual life, but it is, on the contrary, the second which is born from the first" (Durkheim [1893] 1964, p. 279).

As is well known, Durkheim begins his construction with the mechanical solidarity of segmented social units. This segmentary organization has to have weakened before the division of labor can develop. Only after an initial opening up of the segmental insulation of social units from each other and after the emergence of broader solidarities can the division of labor contribute to the further decline of segmental organization and strengthen new forms of solidarity (Durkheim [1893] 1964, pp. 256–57, 275–80). In the light of this idea it becomes clear why Durkheim speaks of "moral density" rather than uses the seemingly simpler and clearer "density of interaction." He refers at the same time to increasing rates of interaction between previously unrelated social units and to the emerging new *conscience collective*.

Second, Durkheim rejects the intervening mechanism that since the earliest formulations of classical economics had been seen as the link between increasing social density and specialization—the gains in efficiency that result from the division of labor. Human happiness, he argues, cannot be endlessly increased, nor does it simply grow as economic resources expand. Human needs and wants, the argument continues, change qualitatively in the process of long-term social change and therefore can hardly serve as the basis for an explanation of such change; finally, even if greater satisfaction of wants as a consequence of specialization led to increased happiness, the anticipation of such consequences could not be a plausible explanation of extended processes of structural change (Durkheim [1893] 1964, pp. 233–55).

For Durkheim, the link between growing social volume and density on the one hand and advances in the division of labor on the other is of a different kind, and this is the third and best known of his modifications of the inherited explanatory argument: the mediating mechanism is competition, which is the more intense the greater the similarity between competing social units. Durkheim uses Darwin's views on the origin of species to explain why greater division of labor follows from increased social density:

If work becomes divided more as societies become more voluminous and denser, it is . . . because [the] struggle for existence is more acute. Darwin justly observed that the struggle between two organisms is as active as they are analogous. Having the same needs and pursuing the same objects, they are in rivalry everywhere. As long as they have more resources

than they need, they can still live side by side, but if their number increases to such proportions that all appetites can no longer be sufficiently satisfied, war breaks out, and it is as violent as this insufficiency is more marked; that is to say, as the number in the struggle increases. It is quite otherwise if the co-existing individuals are of different species or varieties. As they do not feed in the same manner, and do not lead the same kind of life, they do not disturb each other. What is advantageous to one is without value to the others. [Durkheim (1893) 1964, p. 266]

This analysis is applied without further ado to human society: "Men submit to the same law. In the same city, different occupations can co-exist without being obliged mutually to destroy one another, for they pursue different objects. . . . As for those who have exactly the same function, they can forge ahead only to the detriment of others" (Durkheim [1893] 1964, p. 267).³

This is a remarkable argument—and a flawed one. A first observation is that Durkheim identifies all competition with the Darwinian struggle for survival. Though he is not without intellectual company in this view (see Simmel 1890), he certainly goes against the weight of economic analysis, which sees bankruptcy as the fate only of submarginal producers and competition as a relationship that does not normally entail destruction of the other. That is not the major flaw in Durkheim's argument, however. In his transposition of Darwinian ideas to problems of the social division of labor he overlooks the fact that Darwin spoke of a differentiation of demands made on the resources in a given environment—of a specialization of *consumption* as it were—whereas his own problem is to explain the specialization of *production*. This distinction has far-reaching consequences.

It is not at all clear why an increase in population density or an extension of markets should intensify competition among producers.⁴ Increased demand for a product would, *ceteris paribus*, let even marginal producers operate with a profit; their chances of survival would improve rather than diminish. Conversely, increased supply would indeed, other things being equal, intensify competition. What counts is clearly the *relative* development of demand and supply for a given product or service. I fail to see why a relative increase in supply would be the typical consequence of population growth, greater population concentration, advances in communication and transportation, or any combination of these three developments. In fact, the opposite conclusion seems more plausible, namely, that these developments lead more often than not (though not permanently) to rela-

³ Giddens (1978, p. 33) writes that "Durkheim called upon a quasi-Darwinian metaphor," but clearly much more than a mere metaphor is intended.

⁴ Increased population density, due to urbanization and other forms of "concentration of population," and advances in communication and transportation, which allow more far-flung exchange, are the major mechanisms Durkheim discusses as the causes of increased competition (see Durkheim [1893] 1964, pp. 257–60).

tive increases in demand. This assumption is implied in the explanation Durkheim inherited and rejected—the explanation which posited a mutual stimulation of extended market exchange and increased division of labor and, as the theoretical link between the two, productivity gains due to specialization.

One might argue that this critique, while literally correct, misses the main theoretical concerns and ideas of *De la division du travail*. Durkheim, in this opposing view, is grappling with the central theme of 19th-century sociology—the market and its social implications. The main point at issue, then, is a qualitative change: the emergence of competition from a state of monopoly or social exclusion of market competition rather than a change in the degree of competitive pressure.⁵

This is, I think, a quite persuasive argument; it does not, however, do much to salvage Durkheim's causal analysis of division of labor. True, it is possible to supply a theoretical argument—which Durkheim does not make—to the effect that newly expanded markets often have not yet established the patterns of economic dominance or acquired the organizational restraints which in older markets tend to preclude the more intense forms of competition. This line of reasoning could borrow from Max Weber's discussion of the interrelations between status groups and economic classes, interrelations which exemplify the more general problem of how market relations and the social order condition, restrain, and undermine each other (see Weber [1921] 1968, 2:926, 936–38). However, such an interpretation cannot, even with theoretical supplements of the kind just sketched, turn Durkheim's analysis into an acceptable explanation of the division of labor.

First, it is no clearer than before why there should be a special and stable connection between increases in volume and density of population and the breakup of restraints on competition among producers. Simple increases in demand, in contrast to expansive developments which can unsettle established patterns of restraints, may well strengthen, instead of undermining, cartels and other protections against competition. On the other hand, even if it were plausible to assume that increased population

⁵ I thank Alessandro Pizzorno for pointing out this interpretation to me. In the directly relevant passage of the *Division of Labor* (Durkheim [1893] 1964, pp. 268–70), one can find some basis for this reading, though ultimately the interpretation must rely on the overall thrust of the work and disregard several specific arguments. Durkheim does try to construct a scenario of market extension that would change a harmonious coexistence of producers into destructive competition. However, the argument begins with several producers in an industrial center and stipulates nothing about organizational restraints on competition. It derives, incidentally, a highly competitive supply situation from the assumption—introduced like a *deus ex machina*—that “in reality, there is always a considerable number of enterprises which have not reached their limit [of productive capacity] and which have, consequently, power to go further” (Durkheim [1893] 1964, p. 268).

volume and density serve to break down social restraints on competition among producers, there is no reason to see them as the prime factors in unleashing competition. Among the more promising candidates for such theoretical status would actually be the opposite of the developments postulated by Durkheim—a precipitous decline in the demand for a given product, for whatever reason.

Second, the explanatory argument implied by this interpretation of Durkheim reveals the weakness of the dichotomous thinking so popular in 19th-century social thought. It works best in analyzing the consequences of the first emergence of the market—if such a phenomenon can reasonably be conceived. Properly modified (a theoretical strategy typical of Max Weber), it may help in examining repeated threshold effects that occur when, owing to certain pressures and shocks, social restraints break down and open the field for unfettered competition. However, this argument has virtually nothing to say about what pushes division of labor ahead once market competition, in different forms and degrees of intensity, has become a dominant feature of economic life. That means the reinterpreted form of Durkheim's explanation would be irrelevant for the most important causes of division of labor in modern society.

This result leads to my main critical thesis. On a metatheoretical level, Durkheim's critique of utilitarian social theory is fundamentally right, and furthermore, his strictures against that theory open promising avenues for more specific theoretical analyses. However, his contributions to the extended debate over the causes of division of labor are faulty and incomplete, and they are so in large part because of his preoccupation with asserting the antithesis to utilitarian social theory and to its analysis of the division of labor. This applies also to Durkheim's two other modifications of the inherited explanation.

Durkheim's insistence on the primacy of collective life over individual life, the foundation of his thesis that division of labor can take place only within a moral community, has led to crucial insights, for example, that "wherever contract exists, it is submitted to regulation which is the work of society" (Durkheim [1893] 1964, p. 211). More generally, this insight exposes as fundamentally mistaken the implied premise of utilitarian social theory that the institutions necessary for social life can be created at will by contractual agreement among individuals.

However, Durkheim pushes the argument too far when he insists that exchange and division of labor always presuppose an overarching moral community and clear-cut institutional guaranties. His own inconclusive discussion of international trade and the international division of labor attests to this (Durkheim [1893] 1964, pp. 980–82). Max Weber treats what is essentially the same problem in a far more circumspect and his-

torically adequate manner. His starting point is significantly different though informed by the same theoretical concerns: "The 'free' market, that is, the market which is not bound by ethical norms, with its exploitation of constellations of interests and monopoly positions and its dicker-ing, is an abomination to every system of fraternal ethics. . . . At first, free exchange does not occur but with the world outside of the neighborhood or the personal association. The market is a relationship which transcends the boundaries of neighborhood, kinship group, or tribe" (Weber [1921] 1968, 2:637).

As to the emergence of legal guaranties for contractual exchange, Weber posits the following hypothetical construction: "The psychological 'adjustment' arising from habituation to an action causes conduct that in the beginning constituted plain habit later to be experienced as binding; then, with the awareness of the diffusion of such conduct among a plurality of individuals, it comes to be incorporated as 'consensus' into people's semi- or wholly conscious 'expectations' as to the meaningfully corresponding conduct of others. Finally, these 'consensual understandings' acquire the guaranty of coercive enforcement by which they are distinguished from mere conventions" (Weber [1893] 1968, 2:754).

Even within the context of consensual understandings, Weber gives an important role to mutual self-interest as one at least initial guarantee of new arrangements: "The parties to the new arrangements are frequently unconcerned about the fact that their respective positions are insecure in the sense of being legally unenforceable. They regard legal enforceability by the state as either unnecessary or as self-evident; even more frequently do they simply rely upon the self-interest or the loyalty of their partners combined with the weight of convention" (Weber [1921] 1968, 2:756). Durkheim, in contrast, insists dogmatically that association in a moral community has to precede cooperation and that mutual advantage cannot provide a base for the emergence of such association. This, to some, may appear to be a rather technical and even a quasi-metaphysical issue. Its immediate relevance is indeed confined to rather specific analytic problems and it is overshadowed by its metatheoretical, if not metaphysical, import; it is not, however, a negligible issue, even if it cannot rival the preceding or the following one in importance.

Durkheim's critique of the mechanism which in the inherited explanation mediated between increased population density and advances in the division of labor—the productivity gains due to specialization—exhibits the same remarkable configuration of sound metatheoretical argument and flawed analysis of the specific causal conditions of division of labor. He asserts that needs and wants cannot, as utilitarian social theory assumes, be taken as givens; they change historically and are structured by social

and cultural forces. Hence the reference point for judgments about efficiency and productivity is subject to socially structured variation.

If [our ancestors] were so greatly tormented by the desire to increase the productive power of work, it was not to achieve goods without value to them. To appreciate these goods, they would have had to contract tastes and habits they did not have, which is to say, to change their nature. That is indeed what they have done, as the history of the transformations through which humanity has passed shows. For the need of greater happiness to account for the development of the division of labor, it would then be necessary for it also to be the cause of the changes progressively wrought in human nature, and for men to have changed in order to become happier." [Durkheim (1893) 1964, p. 240]

While this is a critique of the utmost importance, in regard to meta-theoretical questions and to the specific issue of explaining division of labor, Durkheim both overextends and underutilizes the idea. His argumentation centers on the impossibility of an endless increase in happiness and the likelihood that happiness did not increase with the commercial and industrial revolution. Yet, these are assumptions the inherited explanation did not need to make. For increased efficiency to be a factor in the situation, the attacked theory merely had to assume that potential demand for goods, services, and unmarketed activities exceeds the supply at any given time, an assumption that can be made in a variety of ways without the postulate of secular increases in happiness as a consequence of growing productivity.

At the same time, Durkheim underutilizes his insight when he simply accepts the proposition that specialization increases efficiency.⁶ What is at issue here is less a question of costs peculiar to specialization, which may outweigh increases in production, than an argument very close to Durkheim's main metatheoretical point—that the efficiency of a procedure is ineluctably tied to a pattern of evaluation or preference structure, which gives specific values to the costs incurred and the output produced (Rueschemeyer 1977). This raises the question of which—or, better, whose—preferences are determinative. Durkheim probably did not ask this question because he was inclined, mistakenly, I submit, to conceive of needs and wants as being fairly uniformly determined by society at any given time; in his view, socially structured variation of needs and wants pertained to intercultural and interepochal differences rather than to divisions within a society.

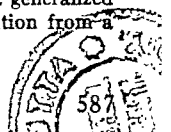
⁶ Accepting this proposition, yet rejecting the idea that prospective gains in efficiency played any role in bringing about increased division of labor, leads Durkheim into awkward arguments designed to explain the higher level of demand which corresponds to greater productivity. He refers to greater fatigue, refined needs corresponding to a greater use of intelligence in work, the fact that new products can activate latent wants, and the attraction of novelty.

Durkheim may also have accepted uncritically the premise of economic analysis that the market assigns values to costs and output that represent a collective preference structure or at least an objective aggregation of individual patterns of evaluation. Clearly, this argument would not hold for nonmarketed activities, which Durkheim intended to include in his analysis. Even for goods and services for which there is a market, it conceals as much as it reveals. Market prices express in aggregate form the preference patterns of those producers and consumers who are able to participate in decisions about consumption and production for a given market, while the preferences of others—employed workers, potential producers unable to enter that market, or consumers not financially equipped to participate—are left out of the market calculus or affect it only indirectly. If we add that markets, which are less than perfectly competitive, give disproportionate weight to the larger market participants, we can see market prices as one among several social mechanisms which insure that the preference patterns of the privileged and powerful determine what is considered more or less efficient in a given situation.

It is possible to combine the two points made and to argue that efficiencies and productivity gains as determined by the interests of the more powerful actors will be an important element in the causal conditions for advances in the division of labor (Rueschemeyer 1977). With this important modification, which derives indirectly from Durkheim's critique of utilitarian social theory, it appears that the classic explanation of division of labor through expanding markets and efficiency gains through specialization is more adequate and more powerful than the "avoidance of competition" thesis Durkheim sought to substitute for it.⁷

This is not the place to sketch a fully adequate explanation of the division of labor or even a program for research on the questions raised above. A few additional issues may be mentioned, however, because they round

⁷ Durkheim criticizes the classic explanation as naively functionalist—to put his argument in words we use today. A passage quoted above continues as shown here: "For the need of greater happiness to account for the development of the division of labor, it would then be necessary for it also to be the cause of the changes progressively wrought in human nature, and for men to have changed in order to become happier. But, even supposing that these transformations have had such a result, it is impossible that they were produced for that, and consequently, they depend upon another cause" (Durkheim [1893] 1964, p. 240). That sounds convincing as long as it is put in terms of changing human nature and a secular increase in happiness. When the issue is reduced to increases in productivity for which there exist market opportunities, the chain that leads from the consequences of specialization back to its causal conditions is no more complicated than in Durkheim's own explanation, which after all also rests on an argument about a consequence of division of labor—namely, specialization reduces destructive competition. In both cases, a combination of rational anticipation, imitation of the successful, selective elimination of the least efficient by competitive pressures, and an assortment of other factors, which a generalized explanatory sketch can treat as random, should suffice to make the transition from a functional to a causal argument.



out the critical evaluation of Durkheim's explanation. Durkheim does not distinguish between different forms and levels of division of labor or structural differentiation. He focuses on occupational role specialization and by implication assimilates other forms of differentiation to that pattern. However, specialization of organization, for instance, may emerge because of causal conditions quite different from those that lead to the specialization of roles, as in the simple example of the emergence of the department store, where role specialization goes hand in hand with organizational differentiation. Furthermore, specialization of occupational roles takes on very different forms if it occurs within an organization and under the control of others instead of among self-employed, autonomous actors. This was the point of Marx's distinction between division of labor in society and division of labor in manufacture, an issue closely related to the role of power constellations in the causation of division of labor (see Rueschmeyer 1977).

Comparing Marx's discussion of the division of labor throughout the second half of the first volume of *Capital* with that of Durkheim brings out another difference, which seems to be of considerable importance for a fuller understanding of the conditions advancing and shaping division of labor. Marx's analysis is far more historically specific. Durkheim had explicitly brushed historical specificity aside. Yet, the search for "constant causes" of the "fact that [the division of labor] advances regularly in history" (Durkheim [1893] 1964, p. 233) should not displace the formation of hypotheses about advances in the division of labor under specified conditions, nor should it make us blind to phenomena of dedifferentiation and fusion of functions whose analysis could be of strategic value in the overall understanding of the causes of division of labor. In fact, Durkheim's assertion of a regular advance of division of labor in history is hardly tenable unless one makes qualifications that take most specificity out of the thesis, and the analysis of broad overall trends will be a fruitless undertaking unless it can build on the analysis of more specific processes.

This discussion has focused exclusively on Durkheim's causal explanation of division of labor. It has not touched on what may well be considered his main substantive concern—the consequences of division of labor for the integration of society and for the autonomy of the individual in a more highly organized social order. Underlying the analysis of both causes and effects of the division of labor is his metatheoretical critique of utilitarian social theory. The thesis of my critical assessment of Durkheim's explanation of the division of labor has been that his metatheoretical arguments are still persuasive, that his causal analysis is faulty and less adequate than earlier explanations, and that, ironically, it was his concern with combating the questionable premises of utilitarian social theory that led him astray at crucial points in his causal explanation.

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Review Essay: A Supply-Side View of the Melting Pot

A Piece of the Pie: Blacks and White Immigrants since 1880. By Stanley Lieberson. Berkeley and Los Angeles: University of California Press, 1981. Pp. xiii+419. \$34.50 (cloth); \$11.50 (paper).

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A century hence, when historians are comparing the progress of blacks after the Civil War with their progress after World War II, the continuities in racial rhetoric will be obvious. Arguments and counterarguments made in immoderate and overblown language in the 1880s have their exact counterparts in the 1980s. What remains uncertain is a similarity of outcomes. After the Civil War, blacks made significant gains that were subsequently reversed. In 1982, despite recent legislative achievements and the improved living levels of many blacks, there remains the nagging fear that what blacks have gained can be lost. Although *A Piece of the Pie* is not directed at the contemporary concern, it will help sociologists understand the social and economic reversals that followed the Hayes-Tilden Compromise of 1876. More than that, this book offers a fragmentary theory of the conditions under which black gains are wiped out.

This is not, however, what the book sets out to do. There are really two books here—the original book that Stanley Lieberson intended to write, and the unintended book that the data eventually compelled him to write. The first book grew from Lieberson's dissatisfaction with conventional answers to the question, Why did the "new immigrants" of 1880–1924 succeed to a greater extent than did black migrants in the North? The second book presents an unconventional and inevitably controversial interpretation of black-white relations. Both books are worth reading, but it is in the second—regrettably much the shorter of the two—that Lieberson's insights and talents shine to best effect.

Lieberson found the standard interpretations of the immigrants' greater success too glib: there were too many explanations, many of them were contradictory or untestable, and the testable ones had rarely been phrased as hypotheses amenable to empirical research. In posing his hypotheses, Lieberson rigorously seeks documentation, combing the decennial U.S. censuses as well as other data sources. Through 11 chapters he contrasts the experience of migrant blacks with that of the new immigrants. However, the treatment is asymmetrical; as the book progresses, the immigrants become increasingly the foils against which the black pattern is displayed.

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The bibliography betrays this asymmetry, for the recent resurgence of multidisciplinary research on the new immigrants and their descendants is ignored. (An alternative explanation for this asymmetry is that the early chapters may have been written years before the 1981 publication date. One clue that this might be the case is the reference to "Ronald Reagan, who moved from movie star to governor of California . . ." [p. 98].)

Perhaps this shift in focus was inevitable, given the problems of analyzing multiple immigrant nationalities with data that are frequently thin. However, it appears that the process of the research also reshaped the question. The issue shifted from one merely of the relative success of the Europeans to the deterioration, even in absolute terms, of the black position. The second book—really only the concluding chapter and a few stray paragraphs elsewhere—emerged as an effort to explain this pattern. This new interpretation, which is a sort of "supply-side race relations" theory, postulates a latent hierarchy of racial/ethnic groups in which blacks occupy the lowest position permanently. As the relative number of blacks increases, a complex set of reactions and feedbacks is put into play to maintain the status quo ante from the perspective of the dominant white group.

The adequacy and the analysis of the data will surely be important themes in the evaluation of this book. Readers familiar with Lieberman's previous work recognize him as a master at discovering clever indicators and presenting them with simple, readily understood measures. Lieberman himself views the data as a principal strength of the work: "One of the pleasures for the author in reviewing the existing statements is knowing that in later chapters the reader will be able to evaluate some of them with relevant data rather than merely leave matters at the level of speculation, conjecture, and—not surprising given the touchy nature of the topic—polemics" (p. 4). This invites criticisms of the places where Lieberman did not use the best data or the best technique. For example, he appears to use published census tabulations exclusively, although public use samples of individual returns are available for two censuses that he uses extensively, those of 1900 and 1960. And regression decompositions and log-linear techniques would in some cases be superior to the techniques used.

But, in my opinion, it is a mistake to see the contribution of the book principally in terms of its comprehensive new evidence. Lieberman is the first to note that the data do not always lend themselves to complicated treatments. And keeping the text readable was obviously an important objective, to the extent that an enormous table of key regression coefficients is presented in a pocket attached to the back cover of the book! The principal contribution of *A Piece of the Pie* lies in the interaction of explanation and evidence as analyzed by an experienced sociologist.

There are few cheap shots and even fewer hasty interpretations in this volume. Above all, Lieberman's scholarship emphasizes the judicious weighing of facts and the assiduous search for alternative explanations. Although they rarely bog down in technical detail, the arguments nevertheless advance slowly and carefully, even ponderously. Speculation is usually identified as such, even to the point that what might legitimately be called

a theoretical proposition is modestly described as speculation. There is no effort to convert historical research into the trendy genre of "it's what's happening now." There is no penny-ante policy analysis tacked on as an afterthought. There is no barely rewritten legislative testimony. (Indeed, in places Lieberman is far too kind to such "scholarship.") Instead, the author draws the reader into serious dialogue, and the arguments and the evidence compel a serious reply. Although a brief review essay cannot do justice to the complexity and detail of the arguments, a survey of the more striking conclusions will illustrate the importance of the work.

I. Immigrants and Blacks: The Standard Interpretations

In what I have called the first book Lieberman sets forth six categories of explanation for the differential success of blacks and the new (i.e., South-Central and Eastern European) immigrants. Regrouped a bit, these explanations are: cultural/normative differences; the legacy of slavery; differences in the timing of migration and shifts in occupational opportunity; residential segregation; and the greater visibility of, and hostility toward, blacks. By no means are these categories mutually exclusive, nor are they equally easy to investigate. All of them assume that success is measured in terms of income, occupational prestige, and access to political power.

Lieberman's approach to the cultural/normative explanations is instructive, not only because that class of explanations is notoriously slippery, but also because it illustrates his research strategy. Cultural/normative explanations of black-immigrant differences argue that immigrants had that ineffable "something" that blacks lacked; some versions argue that blacks have a "fatal flaw" that prevents success. The initial difficulty with these explanations lies in giving operational content to the vague something; the subsequent difficulty is that the unseen critics shift ground to argue that your something is not *their* something, sending you on the endless search for yet another falsifiable hypothesis. Cultural/normative explanations are durable, partly because of their protean nature, and partly because they have a liberal patina that unvarnished biological inferiority explanations do not have. It is ironic but not surprising that the very immigrants now thought to have that something were thought *not* to have that something in 1924; even today, cultural/normative incompatibility is a standard argument against immigration.

Lieberman approaches these explanations by taking stock of the liabilities and the resources of the immigrants and the blacks and by considering the extent to which each of them could be considered cultural instead of structural. (To say that this is what Lieberman did is to oversimplify a complicated argument that requires several chapters, but the simplification is probably not a distortion.) Contrary to the cultural explanation, Lieberman suggests that many of the cultural traits of the immigrants were initially liabilities in the new country. Immigrants were unlikely to speak English, they were from strange and mistrusted religious backgrounds, and they were (at least before 1917) likely to be illiterate. Many resources of

the new immigrants were structural ones. The expectation of life at birth in their homelands exceeded those of Southern blacks—an indicator of relatively greater economic development. Within Northern cities, their infant mortality rates were lower than those of blacks—an indicator of higher living levels. Their home governments and countrymen were able to offer some political support from overseas, and they were able to translate their own growing numbers and their residential segregation into voting strength. Indeed, in many states immigrants were eligible to vote even before their naturalization, while at the same time blacks were fighting a variety of tactics that blocked their access to politics.

In contrast, the liabilities of the blacks were structural ones. Despite their greater familiarity with American customs and language, they had to pursue for decades a political agenda that put them at the starting point of the immigrants. The rights to vote, to hold office, to be on juries, to receive due process and not lynch mob justice, and even to be promoted in a military career were all denied to blacks. Although from time to time some immigrant groups suffered some of these abuses, blacks were always subject to them. Data from union organizers, from Depression-era employers, and from black leaders document the fierce resistance to hiring blacks and certainly to paying them an hourly wage equal to that of white workers.

Lieberson argues that the cultural traits of blacks were, at least initially, important resources. This point is best made by looking at his data on education. Contrary to some stereotypes, blacks were eager to gain education after emancipation. By 1884, over a million black children were enrolled in schools in the South. In most Southern states, black enrollment rates were similar to or in some cases even greater than those of whites, and attendance rates of black children were often better. Although the quality of education in the South was poor, Lieberson claims that the Northern common school was at this time doing relatively little for the foreign-born children. But in the second generation—the black and foreign stock children born in the North—the gap widens, and the common school seems to work for the children of the immigrants but not for the children of the indigenous black migrants.

Lieberson implies that the post-Civil War enrollment rates are indicators of motivation. Later, in the North, substantial gaps in enrollment rates and rates of grade progression opened between the second generation blacks and immigrants. Does this indicate a deterioration of motivation? Lieberson entertains the possibility that the objectively lower rates of return to education for blacks did depress enrollment rates—one may call this lower motivation, or one may call it rationality. But he also considers a “culture of poverty” explanation, for which he examines family stability and the employment backgrounds of parents. He finds that black children were more likely to come from unstable families and that black mothers were more likely to work. However, a decomposition of the enrollment and grade retardation rates by parental employment and family composition attributes substantially less than half of the gap to such variables. Thus,

even in that murky area where structural differences become incorporated into cultural differences, Lieberman finds little merit in the cultural/normative explanations.

There are problems with the argument. It begins by comparing immigrants' children with black children, but the decomposition must be done with white-versus-nonwhite data. Variations in culture among the immigrant groups, especially groups that were already urbanized in Europe, are generally discounted. The long-range incorporation of expectations into a cultural/normative framework is unexamined: we are left with the puzzle that immigrants were apparently more optimistic than blacks, and the fact that subsequent events confirmed the levels of optimism does not explain the puzzle. Nevertheless, Lieberman advances us substantially along the road to understanding why the current explanations of European success are weak or incomplete.

II. Supply-Side Race Relations: Understanding the Position of Blacks

In his second book, Lieberman argues that a larger relative number of blacks initiates white resistance. His insights into the position of blacks come from considering the motivation of the dominant whites. He argues convincingly that the latent structure of whites' racial attitudes is relatively similar regardless of location. North and South, whites are more similar than dissimilar. Furthermore, in a hierarchy of preferences built into this latent structure, blacks are always the least preferred racial/ethnic group. These postulates, as with any postulates, are offered as plausible ones that permit the building of a theory—and that theory, in turn, makes sense of some of the data. Lieberman suggests that the background of slavery and the higher visibility of blacks have something to do with their position, but the postulated preference hierarchy is not itself explained.

To these postulates, add several basic facts. The immigration of undesired foreign nationals, whether they are racial minorities such as the Japanese or ethnic minorities such as the Poles, can be stopped by political action—and indeed, such immigration was sharply limited by 1924 at the latest. But the migration of Southern blacks cannot be legally restricted. As the numbers of black migrants to a Northern city increased, the white majority became more aware both of their numbers and of the impossibility of reversing the migration flow.

At this point, Lieberman hypothesizes, the latent dispositions toward blacks are activated. This results not so much from the desire to "keep blacks in their place" as to preserve the existing conditions for whites. As Lieberman indicates, segregation is not a symmetrical process. As the number of blacks in a city increases, black isolation from whites must increase if white isolation from blacks is to remain the same. In occupational specialties as well as residential segregation, Lieberman demonstrates that an increase in the number of blacks resulted in more black segregation. Moreover, as he argues in his treatment of education, the payoff to whites for segregating blacks increased as the number of blacks increased. Thus, the

fact that black-white relations were historically more oppressive in the South than in the North was related to the concentration of blacks in the South. As soon as blacks began moving to the North, black living conditions in the North began to deteriorate.

This theory, though fragmentary, is provocative, even disturbing. It leaves many questions unanswered. Yet given what he has accomplished in *A Piece of the Pie*, it is unfair to cavil about what Lieberman does not accomplish. Theorists in race relations will find much here that is troubling and that raises still more empirical questions worthy of research. But there is no question that this book is seminal in the most elemental sense; it is one of those important books that sociologists will be reading and arguing about for years.

Book Reviews

Intergroup Processes: A Micro-Macro Perspective. By Hubert M. Blalock, Jr., and Paul H. Wilken. New York: Free Press, 1979. Pp. x+645. \$22.95.

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Intergroup Processes is a gold mine: full of rich nuggets that are hard to dig out. Its objective is to provide models of intergroup relations ranging from micro to macro. Hubert Blalock, Jr., and Paul Wilken first examine microlevel models of choice under the rubric of the subjective expected utility approach. Next, they consider dyadic and triadic processes of exchange and equity. Aggregation into collectivities and contextual effects complete the transition to the macro level where the authors analyze problems of resource allocation and segregation/integration. An examination of dynamic processes completes this complex work.

The approach reflects commitment to operationalism and causal modeling and includes recurrent and detailed examination of problems of measurement. The discussion and the guide to literature would, in principle, permit investigators to translate many concerns almost directly into numerical results. Explicit models of the pattern of relationships among variables are presented in diagrams of boxes and arrows summarizing almost all the chapters. There is frequent consideration of identification, index formation, and dimensionality. The theory building is not allowed to stray from the problems of empirical verification.

This book raises issues that will probably occupy students of intergroup relations for the foreseeable future. These issues are presented compactly in the first chapter and in the opening remarks of many later chapters. A complete theory of intergroup relations must reflect some model of how people make choices among alternatives. Such choices are not "against nature" but must take into account anticipated responses and actual counteractions of others. Much of the causal impact of anticipation counteractions can be examined under the rubrics of equity and power. But then such models need to be embedded in the group contexts formed by the choices of similarly situated others. The pattern of such interdependencies determines the impact of aggregation to a macro level; this is not merely a statistical problem but includes the effects of coordination and normative standards. To replace the term "group" with the variable degree of groupness will require explicit attention to relations within the hierarchy of levels of analysis.

If these are important issues for a complete analysis, they are not simple issues. A stated guiding strategy of this analysis is that complex social

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processes require complex models. The results reflect this strikingly. For example, the summary diagram of the subjective expected utility model developed in the first quarter of the book consists of 41 boxes and 35 arrows. Other sections are equally replete with equations, notation, and diagrams of a comparable degree of complexity.

The result is somewhat uneven. For example, in the section on equity, regarding which the prior literature has made too casual reference to various proportions and ratios, considerable clarification is achieved by formalization and the examination of the resulting equations. In contrast, the chapter on degree and efficiency of mobilization mostly consists of a list of different factors, such as a classification of abstract strategies, with little indication of how these are to be fitted together. It is not a bad list, but it is somehow out of place in a book that opens with a polemic on the virtues of specifying explicit models. The discussion of different pieces of several different processes modeled is often interesting, but it is often interspersed with rather casual discussion of complications that are neither analyzed nor integrated into the overall picture. While some of the analysis results in the crisp, falsifiable statements of theory, other portions have the flabbiness of a conceptual scheme that provides a place for everything without saying much about actual social process.

Ultimately, this book may be most important for what it attempts and for the stimulation it may provide for future efforts. The micro-macro distinction is an enormous analytic convenience, but the behavior of individuals and the actions of collectivities are only different angles of view on the same reality. To achieve consistency among the viewpoints is no simple matter, and the rigorous specificity of mathematical formalism may be necessary. This book reviews and adds to the available tools for this problem. Like Blalock's previous work, it should provide the starting point for a long and potentially rich line of developments.

Understanding Events: Affect and the Construction of Social Action. By David Heise. Rose Monograph Series. New York: Cambridge University Press, 1979. Pp. x+197. \$14.95 (cloth); \$4.95 (paper).

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University of Michigan

David Heise assesses the goals and accomplishments of *Understanding Events* thus: "The ultimate goal in this work is development of an interpretive sociology formulated with mathematical rigor, grounded in empirical procedures, and permitting complex and subtle analyses of social relationships. What is presented now is far short of this" (p. viii). Although this is, I think, an accurate assessment, Heise's effort is serious enough to warrant examining his theoretical assumptions and empirical accomplishments in some detail.

Heise sees sociology as ultimately reducible to the actions and understandings of action by individual participants. Such a Weberian approach probably would have appeared unremarkable some years ago. Today, however, in a theoretical climate where even states can be denied independent status as units of analysis, the approach marks the work as distinctively social psychological. One can do far worse than to construct a good theory of individual action, of course; but one can no longer assume that the discipline will see it as explanation for society. Thus the current appeal of this work is likely to be limited.

With a Weberian interpretive sociology as his goal, Heise sets out an "affect control theory" whose framework and tools are borrowed heavily from psychology. The key underpinning of human perception and social response is taken to be affect, as the book's subtitle suggests. The key dynamic is seen to be action taken to maintain established feelings. In other words, the emotional world is seen as a homeostatic system and the social world as a set of disturbing or confirmatory events impinging on that system. Although the frequently conservative implications of an equilibrium model may make some sociologists uncomfortable, this type of model has retained more favor in psychology than in recent sociology. Heise does a good job of exploring some of the implications of this approach and of tracing its primarily psychological sources.

The mathematical rigor toward which Heise aims includes specification of equations for assessing social actions. (Despite the theoretical interest in actual behavior, the data focus on assessments of described behavior.) Social actions are analyzed into the component parts of actor, action, and object of the act. The key profile dimensions on which each is assessed—again following from a long tradition of psychological research—are the evaluation (good-bad), potency (strong-weak), and activity (lively-still) dimensions. Heise systematically assesses meanings of social actors, actions, and objects, first out of context and then in the context of congruous and incongruous combinations. Relatively straightforward regression analyses can yield answers to questions about how the three profile dimensions affect one another across actor-action-object. Using a catalog of ratings by college students of 650 social identities and 600 interpersonal acts, series of actions between role pairs can then also be predicted (e.g., answers to the questions of which acts were most likely to be directed by father to child; then which by child to father; then which additional act by father, if child had acted).

Of practical importance to many sociological readers might be the logical extension of this work into an analysis of social roles per se. The catalog of ratings for identities is readily adaptable to this purpose, and whole series of exchanges can be generated using this tactic, as exemplified above. Heise details a number of interesting analyses, such as comparisons of deviant and nondeviant roles and analyses of interactions across that boundary; further, he attempts to link this effort theoretically to inventorial, functionalist, and structuralist approaches to roles. This latter effort does

not yield any particularly consistent sociological picture of what a role is or of what its dimensions or components might be. But Heise can hardly be faulted if his dimensions drawn from psychology—which can be and are used to categorize *anything*—work as well and relate in some fashion to most other distinctions that have been drawn about roles. While the reader might long wistfully for a mathematically rigorous analysis of roles in terms of genuinely sociological properties, Heise can hardly be blamed for the field's theoretical confusion. At least, his analysis appears likely to yield sensible predicted configurations of action and emotion among diverse role pairs, an avenue that may prove worthwhile.

If Heise's "affect control theory" is to be applied to such problems as clarification of role relationships, however, its empirical base must be broadened. As he himself acknowledges, it would be dangerous to assume that ratings by University of North Carolina undergraduates in 1975 are broadly generalizable even within our own culture; and these data generally constitute the base for the computer-generated actions and reactions that form the book. Further, despite noting the limitations of the data set, Heise is relatively offhand about interpreting differences based on sex and political attitude even within the restricted college sample. Thus we do not really know the implications of individual and group differences within an already quite homogeneous sample and have no firm bases for predicting effects of other important social cleavages on ratings. And the more important the cleavages prove to be, the more unwieldy the model becomes. On this problem, Heise's model still awaits evaluation.

Overall, *Understanding Events* does what its author sets out to do: it lays out a beginning for a trail of investigation that can contribute to an interpretive sociology.

The Culture of Public Problems. By Joseph R. Gusfield. Chicago: University of Chicago Press, 1981. Pp. xiv+263. \$20.00.

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"Pressed to the wall to solve the problem of drinking-driving I can only shout 'Why do you ask?'" (p. 4). In supplying an answer to his own question, Joseph Gusfield denies neither the role of alcohol in highway accidents nor the need to do something about it. His point is that the research we conduct on drinking-driving and the laws we make to inhibit it tell us more about our moral order than about the effects of drinking-driving itself. Many will object to this conclusion, but none can ignore it. Indeed, the book will put many scientific and legal experts on the defensive as they face Gusfield's massive erudition, pointed analysis and criticism, and pow-

erful argumentation. In *The Culture of Public Problems*, Gusfield presents the experts, and us, with a masterpiece of sociological reasoning.

The public problem of drinking-driving, asserts Gusfield, does not emerge from facts; rather, the problem selects the facts upon which it can base itself persuasively: "Knowledge and law are not shiny marbles lying on the beach and awaiting only the sharp eyes of skilled men and women to be found. The 'facts' of alcohol are picked out of a pile, scrubbed, polished, highlighted here and there, and offered as discoveries in the context of the particular and practical considerations of their finders" (p. 20). These "considerations" have distinct political contours, but they are essentially moral in character. They feed on revelations, not "findings."

If they are to be converted successfully into policy-relevant messages, scientific facts must be organized into persuasive fictions; laws must be dramatically realized in solemn ceremony. To understand this transformation, says Gusfield, we need to exploit the canons of literary as well as scientific criticism. In "The Organization of Public Consciousness" (chap. 2), Gusfield shows us that drinking-driving facts, scientifically derived, are reported less as evidence to compel consent than as rhetoric to induce belief. A complex and uncertain reality of differently weighted and interconnected factors must be condensed into something we can despise. Driving under the influence of alcohol is therefore singled out as a focus of public concern and research. As a major "owner" of this problem, the National Safety Council plays a central role in the definitional process. Categories used and not used in the council's reports betray the general scheme of conceptualization defining the culture of the drinking-driving problem. Accident-prone drivers emerge as the main cause of accidents because data unrelated to the issue of driver competence (like brand and condition of automobile, type of roadway, distance to nearest hospital) are not reported. Nor do we find among the council's official statistics any information on different sources of driver incompetence, for example, fatigue, sedatives, drowsiness-inducing drugs, depression, and emotional trauma. These omissions sustain the self-conception of our culture. Invested with moral impairment by scientific rhetoric, the drinking driver brings the world of accidents to the service of individualism. Causal theories enter into relation with ideological attributions of responsibility.

In "The Fiction and Drama of Public Reality" (chap. 3), Gusfield draws out his argument. Fictive assumptions about blood-alcohol level and driver performance, unwarranted confidence in the adequacy of the samples on which these assumptions are based, and leaps of faith from correlation to causation all conspire to make incompetence a moral issue: the "drunken driver" replaces the "drinking driver." But categorizing as "drunken drivers" those millions of upright citizens who take to the streets daily after an innocent period of social drinking covers too much and deprives drunken driving of any moral significance. Somehow, the man in the gray flannel suit must be set apart from (and above) the "killer drunk."

Rhetorical devices which produce this binary classification of drinking drivers are enumerated in "The Literary Art of Science" (chap. 4). The showpiece is Julian Waller's influential 1967 article, "Identification of Problem-Drinking among Drunken Drivers" (*Journal of the American Medical Association* 200:124-30). The sense of reality and pathos in this report comes from its dramaturgical qualities, which include literary style, agent, purpose, voice and viewpoint, author-audience and distance-subject relations, metaphor, myth, and archetype. By the unwitting application of these literary "tricks," quantitative differences in frequency, extent, and consequences of drinking-driving are polarized and transformed by Waller into a qualitative hierarchy of occasional social drinkers and "real" drunken drivers. By showing that the real drunks are disproportionately black or Mexican, poor, with previous arrest records, Waller makes the hierarchy all the more absolute.

In the next two chapters—"Law as Public Culture" and "The Legal Myth of Social Order"—Gusfield's thesis takes a new turn. Morality, he explains, has two sides: restraint and remission. While sobriety is associated with the restraints of safety, sociability, altruism, control, plannedness, obedience, wisdom, seriousness, and work, the tensions occasioned by these values must be released periodically through behavior, often alcohol induced, associated with danger, unfriendliness, hedonism, lack of control, spontaneity, disobedience, foolishness, frivolity, and play. These remissions account for the immense volume of drinking-driving offenses which inundate the nation's courts daily and which demand and get from these courts a policy of accommodation and leniency. Thus at the formal level, in *written law*, the distinction between drinking-driving and other traffic offenses is clear-cut. At the practical, situational level the distinction is ambiguous.

The leniency with which drinking drivers are treated is part of a concordat struck between ideal and circumstance. But in this compromise the ideal is nonetheless compelling. Dramatically heavier penalties provided for drinking-driving do establish a public vision of what is right and proper; they do embody an image of society in which moral standards are embraced and protected. Law, declares Gusfield, is more than a mechanism for achieving compliance; it is a form of communication which conveys a view of public order that is certain and coherent:

Legislation and judicial decisionmaking are seen as public culture. They create a version of a consensual "society" which is ordered, consistent, and predictable. In doing so they support a moral order and a cognitive order, ritualistically repeated and supporting enforcement as public authority. The public culture of drinking-driving maintains the acceptance and indisputability of a culture of rational, responsible risktaking and the evil of self-expressive, hedonistic action. When ordinary traffic infractions are sharply distinguished from drinking-driving, the use of alcohol is given a special status; it becomes morally deviant, and auto accidents become results of moral turpitude. [p. 188]

This gloss on the sociology of law brings the book to a familiar (Durkheimian) conclusion.

Although Gusfield sees dramaturgical analysis as his main research tool, that method should be distinguished from the theory which animates its use. Dramaturgical concepts contribute a respected vocabulary for the expression of ideas actually derived elsewhere.

On the back cover of this book, there is an advertisement for Marshall Sahlins's "related book" on *Culture and Practical Reason*. That is an appropriate reference for those who might be interested in the full intellectual background of Gusfield's work. From Sahlins we learn that the most expensive commodities are not objectively superior to less costly goods. Symbolic constitution, not utility, determines demand and price. Thus different cuts of meat and clothing are "good to think with" because their differential consumption helps sustain the hierarchical boundaries of society. Just so, the elements of driver incompetence that we despise most are not those that contribute most to highway accidents but those that most readily symbolize what society both cherishes and abhors. Different "causes," then, are packaged symbolically in different ways and so lend themselves more or less well to the formulation of public policy. Dramaturgical analysis is a good method for articulating this connection; however, it does not sensitize us to the source of the connection itself. The work of Sahlins and other symbolic anthropologists, like Claude Lévi-Strauss, Mary Douglas, and Victor Turner, speaks loudest to this issue. Though explicitly recognized by Gusfield, their work is not central to his argument. He sees the correspondence between moral and cognitive dualism, between binary visions of social ideals and polar typologies of drinking drivers, but does not try to assess the quality of that correspondence. The depth and permanence of the connection are not brought out as central issues. As a result, the full impact of Gusfield's conclusion is muted.

At a time when our discipline is concerned with representing itself as a champion of rational social policy, Gusfield demonstrates how this program can get off the track. His discovery is nothing less than brilliant. We think our policies are grounded in fact; in reality, our facts are based on preformulated policies. That reality subsists for reasons of its own. If our scientific and legal rhetoric produces an image of the world that is clear, understandable, and guided by universally applied standards, if it establishes in our minds the conception of a good and orderly society, then only demoralizing consequences would obtain from a different kind of rhetoric. The relation of science to fact is thus shaped by moral imperatives that are not of science's own making. Expressed in the law of drinking-driving, science becomes a ritual instrument of cultural affirmation rather than a source of objective knowledge. The issue that Gusfield's great book raises, but leaves for others to address, is whether we can persuasively link social science to social policy in any other way.

Acceptable Risk. By Baruch Fischhoff, Sarah Lichtenstein, Paul Slovic, Stephen Derby, and Ralph Keeney. Cambridge: Cambridge University Press, 1981. Pp. xv+182. \$19.95.

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How safe is safe enough, and how are we to decide what is safe enough and what is not? If, on the average, reactor accidents decrease a person's life expectancy by two days, while cigar smoking reduces it by 330 days, and being an unmarried male reduces it by 3,500 days, does this mean that our reactors are safe enough? If human lives are estimated to be worth \$200,000 (this is the lowest estimate given) and Ford Motor Company calculates that it will come out ahead financially by manufacturing Pintos with an inexpensive fuel tank design known to increase the risk of death in rear-end collisions, does this mean that Pintos really were safe enough? And if one tries to overdesign a bridge so that it will be strong enough to withstand any conceivable load, does this mean that one can safely disregard uncertainty about future traffic patterns? Baruch Fischhoff, Sarah Lichtenstein, Paul Slovic, Stephen Derby, and Ralph Keeney do not provide any answers to these questions in *Acceptable Risk*, but they do suggest how such questions might be decided and how to evaluate the decision processes.

The authors define their subject as acceptable-risk problems. An acceptable-risk problem is a decision problem like any other except that at least one of its alternatives includes a threat to life or health among its consequences. The trick is to decide whether *in this context* the threats to life and health are acceptably low. The kinds of problems with which the authors are concerned include decisions about whether to ban saccharin, whether and where to build nuclear reactors, which birth control methods to use, and how to design safer airplanes.

They begin by reviewing some of the reasons such decisions are hard to make (including, e.g., decision makers' unfamiliarity with terms used in technical debates—terms like "megadeaths"). They next outline a set of criteria that might be used in choosing an approach to an acceptable-risk decision; such decision procedures should be comprehensive, logically sound, practical, open to evaluation, politically acceptable, compatible with existing institutions, and conducive to learning.

The authors use these criteria to evaluate three decision procedures: professional judgment, "bootstrapping," and formal analysis. By professional judgment, they seem to mean the conclusions of those technical experts most knowledgeable in the field. Professional judgment integrates the personal experience of the professional, accepted professional practice, and the desires of the client. Examples of professional judgment include a physician's decisions about the treatment of his patients, design standards set by engineering organizations, and the performance standards contained in such governmental codes as the Clean Air Act. Like all decision procedures,

professional judgment has its shortcomings, and the authors argue that, in practice, the conclusions of professionals are not open to evaluation by outsiders because these conclusions are grounded in esoteric knowledge gained through apprenticeship. They also fault professionals for ignoring the roles of human beings in the systems they design, trying to compensate for human underdesign with technical overdesign.

Under "bootstrapping" the authors include all those decision procedures which look to the past for guidance and aim at decisions that are consistent with those of the past. Bootstrapping techniques thus include revealed preference, implied preference, risk compendium, and natural standards decision methods. The virtue of such decision techniques is that they circumvent cumbersome decision processes and disputes by abstracting the standards implicit in past decisions and then applying these standards to current decision problems. One of the chief defects of such procedures is that they assume that the past is a good guide for the future, either in the sense that past death rates were low enough or that past distributions of costs and benefits are still acceptable.

The third decision method considered by the authors is formal analysis. By formal analysis they mean those techniques (like cost-benefit analysis, cost-effectiveness analysis, and decision analysis) that try to list all the advantages and disadvantages of a set of proposed actions and to choose the *best* solution or at least to show which alternative is best, given the elements that are included in the analysis. Among the advantages of this method, the authors argue, are its comprehensiveness and its openness to evaluation. But since many of the components of such analysis—for example, in the nuclear reactor design evaluations—are professional judgments, which the authors have already told us are inscrutable, most formal analyses amount to aggregated inscrutability.

In a subsequent chapter the authors compare their assessments of these three decision methods (and note that there are other methods that they have not evaluated), rating each method on each of the criteria listed above. They then, too briefly, suggest that which method will work best will vary from situation to situation depending, for example, on whether the decision to be made affects only a single individual or the whole society, whether it has to do with a component of a technology or the technology as a whole, or whether it is a decision about a novel technology or an old one. The authors also provide recommendations for improving public participation, for making the work of technical experts more accessible, for offering safety as an option in market transactions, and for upgrading government intervention. They point out areas of particular ignorance and argue that there will be substantial benefits for the society as a whole if we invest more in research about acceptable-risk decision making.

Though it is filled with interesting facts (riding a motorcycle for an hour is as dangerous as being 75 years old for an hour; life expectancy is reduced by 16 minutes by each airplane takeoff or landing), provides a good introduction to some literature about which many sociologists are ignorant

(e.g., recent work in psychology on perception of risk), gives a cautious and balanced account of some of the difficulties of decision making, and is written in a lively style ("The fact that no major dam in the United States has been left unfinished once begun shows how far a little concrete can go in defining a problem" [p. 13]), this odd little book is ultimately unsatisfactory. On the one hand, it aspires to be a consumers' guide to decision-making methods, but on the other it is a treatise on the need for a new academic field (acceptable-risk decision making).

As a consumer guide, the book will probably prove unsatisfactory because its coverage of key problems is too superficial. If you do not already know how standards are set and what relation standards bear to professional judgment, you will not find out by reading this book. It is even unclear precisely who are "professionals," what kinds of people perform risk analyses, or who employs the bootstrap method. A few sustained examples of each type of procedure would help a lot.

Unfortunately, the book makes its case for the interdisciplinary nature of the field negatively as well as positively. Academics will find that the authors neglect bodies of literature that are central to their concerns. If ever there were a case to which March and Simon's conclusion (that decision makers satisfice rather than maximize applied, this is clearly it, but the authors fail to discuss *organizational* limits on either calculative or adaptive rationality. When they discuss the problems of making decisions that affect the entire society, it is somewhat surprising that they neglect the literature on collective decision making and the collective irrationalities that can result from aggregating individual preferences. In discussing areas as fraught with conflict as nuclear power, the authors do not discuss either class conflict or interest group processes. Such sociological phenomena are clearly as important in determining the ultimate effectiveness of a decision-making method as the technique itself.

If a field of acceptable-risk decision making does emerge, one can safely bet that this book will be the classic work read by everyone in the field. But it is still worth reading now.

Power in Organizations. By Jeffrey Pfeffer. Marshfield, Mass.: Pitman, 1981. Pp. xiv+391. \$19.95 (cloth); \$9.95 (paper).

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For some time organizational scholars have been bothered by their inability to analyze power and politics in organizational settings. That shortcoming has been made less severe by the publication of Jeffrey Pfeffer's political approach to organizational analysis. *Power in Organizations* represents an important synthesis of the literature on power, a synthesis grounded in the

sociological assertion that power is basically a structural phenomenon resulting from the division of labor. Pfeffer's argument spreads across nine chapters and covers the assessment, use, creation, perpetuation, symbolizing, and managing of power as it flows through decision-making processes in organizations.

The heart of Pfeffer's analysis is the proposal that power and politics vary as a function of the interdependence of differentiated units, heterogeneity of goals, heterogeneity of beliefs about technology, scarcity of resources, importance of the issues involved, and the dispersal (rather than centralization) of power. As these five variables increase, organizations are likely to become more politicized.

A flavor of the insights that flow from Pfeffer's analysis is evident in the following excerpts:

1. "The power of organizational actors is fundamentally determined by two things, the importance of what they do in organizations and their skill in doing it" (p. 98).

2. "Interestingly enough the push to implement such [rational] management practices [as linear programming, PPBS, zero-based budgeting] is often intensified during times of financial problems or resource scarcity. Thus, just at the very moment when the decision making process is likely to become even more politicized because of the increasing level of scarcity, an attempt may be made to impose a procedure requiring assumptions of goal consistency" (p. 95).

3. "Failure [of a policy] is justified to argue for more resources [e.g., we're doing the right thing, we just haven't done enough of it] and many of these resources are provided to those in power to enhance the implementation of policies already undertaken; these resources, then, serve to further institutionalize the power of those already in the dominant coalition in the organization. Indeed, nothing may succeed like failure, at least for a while" (p. 298).

4. "Ironically, at the very time the organization faces a crisis which may have resulted from inattention to the demands of the environment or from failures of policy, psychological dynamics [such as tightened control and centralization of authority] are set in motion which reduce contact with the external environment, place a premium on loyalty and commitment to already chosen policies, and define as treasonous any kind of deviation from the adopted line" (pp. 324-25).

5. "Among the critical skills which are needed by managers who are filling administrative roles in organizations in which power and politics play an important part are a tolerance for uncertainty and ambiguity and the ability to confront and manage conflict and advocacy. Training in advocacy, case preparation and presentation, and power and politics including the use of symbolic acts and language, may be more appropriate training for the manager in a political organization; at least such skills can be useful as supplements to the technical analytic skills that seem now to dominate business education" (p. 354).

The most striking feature of this book is that it has much wider value than its title implies. This is more than just a book about power and influence. It is a thoughtful, precise introduction to the study of organizations in general. The first four chapters, for example, provide one of the best available introductions to the field of organizational analysis. Chapter 1, by itself, informs the reader of four major perspectives on organizations (rational decision making, bureaucratic, decision process, political) and indicates where each makes predictions not handled by the other three. In addition, the first chapter introduces the tension between applied and basic work, describes political issues that infuse the field itself, and illustrates ways to differentiate the four positions on issues where they seem to be saying the same thing.

What is even more remarkable is that, even though the book avowedly employs a macro perspective grounded in basic empirical sociology, it is extremely strong in its treatment of social psychology (e.g., commitment, informational influence, escalation); process variables (e.g., tactics of power utilization); cognitive accompaniments of structure (e.g., definitions of the situation); and organizational change (e.g., institutionalization reduces adaptability).

This book is neither derivative nor merely eclectic. It has a distinctive voice. The chapters cohere and are tied together by common themes. There is an emphasis on accurate exposition that is evident in the detailed outlines that organize sections, the informative chapter headings, and the avoidance of clever but opaque phrases. Examples are abundant and tend toward settings that even researchers who have not ventured outside academe are familiar with (e.g., schools of management, university budget making, professional societies).

With the publication of this book the field of organizational studies now seems to have a critical mass of literature that provides an accurate representation of the field. Relevant items include: R. Scott, *Organizations: Rational, Natural and Open Systems*; J. Pfeffer and G. Salancik, *The External Control of Organizations*; J. March and J. Olsen, *Ambiguity and Choice in Organizations*; H. Aldrich, *Organizations and Environments*; J. March and H. Simon, *Organizations*; R. Hall, *Organizations: Structure and Process*, 2d ed.; M. Meyer, *Environments and Organizations*; J. Galbraith, *Designing Complex Organizations*; K. Weick, *Social Psychology of Organizing*, 2d ed.; Robert Miles, *Macro Organizational Behavior and Coffin Nails and Corporate Strategies*; B. Staw and L. Cummings, series on *Research in Organizational Behavior*; A. Van de Ven and E. Joyce, *Perspectives on Organization Design and Behavior*; G. Allison, *Essence of Decision*; W. Ouchi, *Theory Z*; R. M. Kanter, *Men and Women of the Corporation*; B. Nystrom and W. Starbuck, *Handbook of Organizational Design*; and forthcoming volumes by T. Peters, M. Hannan and J. Freeman, and W. McKelvey. Given the existence of this critical mass of resources in the understanding of organizations, this seems like the right time to initiate "new wave" organizational studies that begin to edit, prune,

apply, and synthesize what we have in order to see what we do not have yet, what we have too much of, and what we agree on as the basics. What makes this assessment plausible is the fact that Pfeffer has shown that the several perspectives now dotting the field can converge more strongly on a problem than mere lists of properties can.

The legacy of Graham Allison's analysis of the Cuban missile crisis has been the implication that to study organizations by using multiple perspectives requires attaching the perspectives, one at a time, to see what they do and do not highlight. And the result of that sequential one-at-a-time application is the analysis. Pfeffer does not do that. He applies multiple perspectives simultaneously to a durable puzzle in organizations and shows not only that power is less puzzling than we thought but also that all perspectives we are currently working with have something to say about how that puzzle can be untangled most meaningfully.

In doing so, Pfeffer has made a major contribution in the form of an important book.

Corporate Control, Corporate Power. By Edward S. Herman. New York: Cambridge University Press, 1981. Pp. xv+432. \$18.95.

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Debate over the separation of ownership from control has framed most of the theoretical and empirical work on the issue of power and structure of the modern corporation. Initially articulated in 1932 by Berle and Means, the idea that managerial control has replaced the prerogatives of owners has found expression in theories of organizational behavior, social stratification, and political sociology.

Managerial theory has been firmly buttressed by input from organizational sociology. If management is freed from the dictates of stockholders, then corporations can be equated with bureaucracies where organizational interests rather than profits become the overriding concern. This way of articulating the issue has had profound consequences for political sociology and stratification theory. For if there is a non-owning managerial class that oversees the large corporations, then the distribution of economic power would seem to be pluralistic, and organizational position and expertise rather than property relations would represent the key stratification dimensions of American society.

Major contributors to this debate have tended to take sharply opposing positions, either asserting the autonomy of non-owning managers (Bell, Burnham, Dahrendorf, and Galbraith) or directing their efforts to documenting the continued importance of family ownership and growing domi-

nation of firms by a few large financial institutions (Burch, Kotz, and Zeitlin). In *Corporate Control, Corporate Power*, the most recent and comprehensive reassessment of the Berle and Means thesis, Edward S. Herman musters an imposing body of evidence to argue forcefully that this sharply drawn dichotomy between the interests of owners and the goals and behavior of managers is quite misleading. The "managerial revolution" did indeed occur, but it did not produce the effects predicted by managerial theory.

Herman analyzes the evolution of corporate control from 1900 through the mid-1970s and classifies by type of control the 200 largest nonfinancial corporations in 1974-75. He finds a steady increase in management control from only 23% around 1900 to 82% in the mid-1970s. Correspondingly, he finds a sharp decline in direct ownership and outside financial control of the largest corporations.

Herman's classification of firms according to type of control gives a much lower estimate of family, bank, and intercorporate control than several previous studies, most notably those of Burch, Kotz, and Zeitlin. Estimates are, of course, partly products of initial definitions of control, and Herman tends to emphasize the effective exercise of authority over operations and capital flows instead of the latent sources of power. An astute observer of the business press, Herman bases his theory of control by "strategic position" within the organization on a detailed analysis of the composition of corporate boards and their interactions with management. Corporate boards, he finds, are generally passive units that serve as forums for exchange of information rather than as arenas of challenge to inside management. Concerted actions by stockholders and creditors against management are the exception rather than the rule.

These findings pit Herman against both apologists and critics of postindustrial society theory. Against the instrumentalism of many left-oriented critics, Herman argues that, despite increased economic concentration and centralization, the American economy is not controlled by a few powerful financial institutions or families. For Herman, corporate control is largely exercised, neither through pressure by boards nor by direct intervention by powerful owners, but by the market effect of individual and institutional stockholders and creditors seeking acceptable rates of return on their capital investment. Herman thus offers a structural view of corporate control, one that emphasizes the role of the market in determining corporate behavior.

The main thrust of his argument, however, is directed against such theorists as Galbraith and Earl Kaysen, who have argued that corporate managers have been freed from the immediacy of proprietary interests. In sharp contrast to the vision of a "soulful corporation," Herman proposes a "constrained managerial" theory of corporate control. Corporate executives frequently have sizable though not controlling stock holdings in the firms they oversee. But more important, there are two faces of managerial autonomy. If the separation of ownership from control has, on the one

hand, given managers autonomy from direct intervention by outside interest in key corporate decisions, it has, on the other hand, rendered these same managers subservient to the constraints of a highly institutionalized system of rational profit seeking. This prison-house of profits, the modern corporation, substantially limits and carefully circumscribes managerial discretion and potential abuse by tying executive compensation and promotion to short-term rate-of-return considerations. Herman finds little support for the idea that managers emphasize growth in sales and earnings per share at the expense of rate of return to investors. Despite growth in size and complexity, corporate organization remains fundamentally capitalist in goal and in practice; it has not become a neutral self-perpetuating bureaucracy.

The idea that managers are no different from owners in terms of interests and behavior had been previously argued by critics such as Mills and Miliband. But in the hands of Herman it receives a spectacular documentation. Moreover, just as large corporations are not directed by the coordinated efforts of a small corporate elite, they also are not malleable by liberal reformism. Proxy fights, disclosure, and changes in board composition are unable to mitigate the underlying drive for profits. The all-embracing profit orientation renders the first unnecessary and the second illusory.

In a chapter on big business and government regulation, Herman documents precisely what Miliband, from a Marxist perspective, several years ago called "imperfect competition" and what Charles Lindblom, from a non-Marxist perspective, more recently refers to as the "privileged position of business." Herman shows that though perceived by business leaders as costly infringement upon their prerogatives, the formal regulatory powers of government do not curtail business power fundamentally: regulatory measures are not that comprehensive and go unapplied to a considerable degree.

Herman's structuralist framework—one that focuses on the constraints imposed by the logic of profit making on firms and managers—seems, however, ill suited for addressing an issue that would seem central to a book about corporate power, namely, the capacity of the capitalist class to mobilize itself as a political actor. For in reading Herman, one might well come away with the impression that the managers who preside over America's mammoth corporations are so locked into a system of profit making that they have neither the interest nor even the need to act in concert in the larger political arena: a proposition that flies in the face of an impressive body of research on the capacity of business to mobilize in defense of its interests.

Nevertheless, Herman's work will undoubtedly find its place as one of the classics in the field of study of corporate leadership, power, and structure. It is an exemplary piece of scholarship that synthesizes with remarkable clarity an impressive array of data from a wide variety of sources. Its contribution is decisive. As a result, managerial theory, at least in its classical formulation, can no longer continue to set the terms of the debate over power and structure in the modern corporate order.

Bureaucracy and the Labor Process: The Transformation of U.S. Industry, 1860-1920. By Dan Clawson. New York: Monthly Review Press, 1980. Pp. 284. \$16.50.

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Dan Clawson's *Bureaucracy and the Labor Process* has received all too little attention. That is probably because he packaged it so poorly, but nonetheless it is a shame, because the book contains some important and well-done research.

Clawson studies the shop-floor organization of American factories between 1860 and 1920. His argument is that "an inherent tendency of capitalist development [pushes] capital to take more and more control over the work process. One of the main purposes throughout the book is to document . . . this position" (pp. 27-28). The book analyzes the transition from a situation (ca. 1860) in which employers, because they knew so little about actual production methods, necessarily left much of the detailed control over work to the workers, especially the skilled ones, to a new system (installed by 1920) in which a supervisory hierarchy successfully dominated the labor process. This transition, as Clawson rightly notes, was neither easy nor smooth.

In the first two chapters Clawson reviews some of the new wave of labor process studies by Marxists and places himself in this growing tradition. The next three chapters present his historical research on, respectively, inside contracting, control in craft production, and the erosion of these forms of "workers' control." He then inserts a chapter on Frederick Taylor and scientific management. The book ends with a chapter noting the "barbarism" of present capitalist work organization and the need for a workplace struggle for socialism.

The merits of this book are easy to discover. Chapters 3 ("Inside Contracting") and 4 ("Craft Production") contain the best available description of how American factories were governed during the latter decades of the 19th century. It is lamentable that sources for shop-floor relations are so spotty and unrevealing. But Clawson has done his work well, and we are rewarded with an argument that is detailed, comprehensive, balanced, and persuasive—in short, authoritative. On the basis of surviving payroll records he shows, for instance, the widespread use of inside labor contractors, who often attained exceptionally high incomes (incomes that eventually provided one incentive for capitalists to abolish inside contracting). He explores the mutually dependent and contradictory relations between inside contractors and, on one side, capitalists and, on the other, workers. The power of Marxian class analysis, embedded in a systemic dynamic, provides rich insights here. Chapter 4 is less original, but the discussion of craft control is highly useful. These chapters should become a standard reference.

Unfortunately, the flaws in the book are all too evident as well: the chapters that surround Clawson's detailed empirical work are neither persuasive nor easy reading. The problems begin with the title page: the "bureaucracy" of the title is quite misused. It does not refer to that type of organization governed by formal rules, specified duties, and officeholders who occupy defined "positions"; instead, Clawson uses it to mean hierarchy. But to insist that all hierarchy is "bureaucratic" is to lose the meaning of bureaucracy. Second, there are the obligatory quotes from Marx sprinkled throughout, showing the correctness of Marx's analysis even where others (e.g., Lenin) have strayed from the course. Marx, the master historical investigator, is shown to be above history himself.

Other problems could be cited as well. The assumed relationship between inside contracting and (nonhierarchical or "nonbureaucratic") "workers' control" is problematic at best. The various discussions of capitalist reality and socialist ideals avoid the hard questions of socialist reality.

But perhaps the most curious problem concerns the discussion of Frederick Taylor and scientific management. Here Clawson seems to change his method of analysis in order to romanticize Taylor. For instance, in defending the notion that technology is shaped by class struggle (a good Marxian idea), Clawson (p. 66) approvingly quotes Paul Mantoux: "An idea which flashes suddenly into the mind of a genius, and whose application produces no less suddenly an economic revolution, is what we might describe as the romantic theory of invention. Nowhere do we find evidence of such creations *a nihilo*, bursting forth like miracles, which only the mysterious power of individual inspiration could explain." Yet when it comes to scientific management, Clawson overthrows this Marxian (and Mantouxian) insistence on history as driven by mundane processes involving large numbers of largely unknown people. We learn (p. 30) that "Frederick Taylor was the capitalist genius, who not only recognized the problem [i.e., of workplace control] and devised a solution, but himself led the struggle to introduce the new way." Elsewhere Taylor's "two-fold genius" is extolled (p. 255); and even his excellence in "three different respects" (p. 202); his "unique" personal background is asserted to be "crucial for the development of his system" (p. 210); and his "unique place" in the whole management history explained (pp. 202-3). Why does Clawson, who like other good Marxists usually spurns the "Great Man" theory of history, bend the rules to the breaking point in defense of Frederick Taylor's reputation?

It should be obvious by now that this paean to Taylor has little to do with the man's true historical role, about which reasonable people may argue. No, Clawson suspends his self-imposed rules of evidence not in defense of Taylor, it turns out, but in defense of Harry Braverman. Braverman, in his widely discussed *Labor and Monopoly Capital*, made Taylor the centerpiece of his argument and used Taylor's writings as his chief evidence—evidence not of what Taylor said (or wrote) but rather of what was allegedly occurring on the shop floor. A neat trick, since it obviated

the need for all that bibliographic footslogging of the type at which Clawson (in chaps. 3 and 4) has excelled. Braverman's followers, among them Clawson, have thus inherited the burden of defending this uncharacteristic (for a Marxist) Great Man explanation. Such inconsistency does not seem to embarrass the author.

My conclusion is twofold. First, this book shows both the rewards and the frustrations for readers, especially Marxist readers, of the emerging Marxist literature. Second, you should not let the author of chapters 1-2 and 5-7 of this book prevent you from reading chapters 3 and 4. This packaging around Clawson's real and, I trust, lasting contribution is dross; but chapters 3 and 4 are the right stuff.

Labor Relations and Multinational Corporations: The Cerro de Pasco Corporation in Peru (1902-1974). By Dirk Kruijt and Menno Vellinga. Assen: Van Gorcum, 1979. (Distributed in the United States by Humanities Press.) Pp. xviii+262. Dfl. 45 (paper); \$25.75 (U.S.).

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There are very few concrete studies of industrial relations in Latin America. *Labor Relations and Multinational Corporations* is a welcome contribution to that literature. It is a study of Peruvian copper miners who work for the American-owned Cerro de Pasco Corporation.

Contrary to the predictions of earlier work on labor movements in Latin America, there has not been a trend toward greater institutionalization and moderation in industrial relations in Peru. Strikes in the copper mines have been frequent and violent, the mass of the mineworkers appear to be highly politicized, and the unions have engaged in direct confrontations with the state as well as with management. Dirk Kruijt and Menno Vellinga seek to account for these developments by formulating a theory of the development of class consciousness and class conflict which emphasizes the enclave nature of the mining sector. The fact that mining is an enclave and an important earner of foreign exchange means that mineworkers are able to create solidaristic unions that have tremendous strategic significance in the economy. This necessarily involves tensions with the state, and the history of unionism in the Peruvian copper-mining industry is marked by a complex cycle of politicization, confrontation, and repression interspersed with attempts by the state to control directly or to co-opt the trade union structure.

The book has a useful historical introduction, outlining the history of copper mining in Peru, with attention to both the economic linkages within the industry and the historical formation of a mining proletariat.

The second half of the book is an examination of the pattern of recent

industrial relations in the mining industry. Using a variety of data sources, including company personnel files, the authors employ their material imaginatively.

The discussion of strikes and industrial relations in the Cerro de Pasco Corporation includes a very interesting analysis of union leadership. The authors conclude that the Michelian thesis of increasing oligarchic domination of the mining unions is incorrect and that there are important countervailing pressures. Their analysis stands out as a beacon of light amid the almost complete absence of empirical inquiry about the internal workings of labor unions in Latin America.

Unfortunately, the lack of systematic data at the individual level of the kind that might have been generated by a sample survey or by participant observation inhibits their ability to deal with such questions as the development of class consciousness and the links between miners and their rural communities of origin. Of course, all research designs are limited by available resources. Nevertheless, this restriction in data sources means that certain important issues are not dealt with in a way that is likely to command universal assent; some parts of the authors' argument are thereby weakened.

For example, to account for differences in strike propensities within the copper-mining industry, the authors focus on the nature of the mining camp as an explanatory variable. They are unable, however, to explore an alternative hypothesis (that differences in strike propensities derive from differences in the social origins of miners) because they lack appropriate data. As a result, some readers will feel a certain reserve about Kruijt and Vellinga's conclusions.

Furthermore, we would like to know much more about miners' attitudes and the development of class consciousness. Despite the authors' imaginative use of songs and newspapers, this is a major lacuna in the study. Clearly, there is scope for further work in this area.

Their theory of enclave labor relations is also likely to generate some controversy. Some reference to comparable situations in Chile and Bolivia might have gone some way toward dispelling my feeling that the theory advanced in this book may be difficult to apply to countries other than Peru. Much of the political dynamic analyzed by Kruijt and Vellinga derives from the fact of intense competition by the populist Alianza Popular Revolucionaria Americana (APRA), the Communist party, and the radical Left for the allegiance of the mineworkers in the context of intermittently open politics. It is by no means clear that such a situation has prevailed in other mining enclave situations. Further, on the issue of generalizability, the book's title is something of an exaggeration. Restricted as the study is to a mining enclave, what it can tell us about labor relations in manufacturing industry is quite problematic.

Nevertheless, when all is said and done, the verdict on this book must be a positive one. Theorizing in this area has been hampered by the paucity of empirical material. Kruijt and Vellinga's contribution is both meth-

odologically and theoretically sophisticated and will be essential reading for scholars working in this area.

Trends toward Corporatist Intermediation. Edited by Philippe C. Schmitter and Gerhard Lehmbruch. Beverly Hills, Calif., and London: Sage, 1979. Pp. 328. \$22.50 (cloth); \$9.95 (paper).

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Trends toward Corporatist Intermediation is a collection of 10 essays with an introduction and conclusion by the editors, Philippe Schmitter and Gerhard Lehmbruch. The essays begin with Schmitter's widely quoted and well-known "Still a Century of Corporatism?" and include contributions by scholars from the United States, Canada, and Western Europe on such topics as bargaining by political elites as a response to "fragmented political culture" (p. 53), class conflict and corporatism, the history and types of corporatism, analyses of changes and development of corporatism in specific cases, relationship of corporatism to theories of the state and political culture, corporatism and the future of parties and parliaments, problems and conditions of consensus building, U.S. and Japanese exceptionalism, and the criteria for inclusion or exclusion of types of interests in the state.

The purpose of the collection is to bring together articles in order to define and refine "corporatism" and to develop a working methodology for studying it. The authors of the essays appear doubtful about developing a common theoretical orientation, although most seem to agree that theory must guide methodology and vice versa. Indeed, the collection includes decidedly contradictory theoretical frameworks (although there are no born-again pluralists among them). Even though a number of the authors try to develop a conceptual framework to differentiate empirical types and discriminate theoretical positions, none of them presents a framework which I can envision being adopted by all of the others.

Corporatism, which refers to organized special interests joining together to make policies within a country, is of two types according to Schmitter's definition. State corporatism, which embodies the image of fascism, is the more familiar and older conception. Societal corporatism, the newer and apparently more interesting conception, embodies the image of social planning. The new corporatism, although different, raises fears of a return to earlier oppressions, along with promises of superseding them. What the "new" corporatism has that the earlier versions lack is the participation of labor in the negotiations (with the exception of Japan as suggested by the Pempel and Tsunekawa article). Nevertheless, the fear is that perhaps there is no difference, because the incorporation of labor presents the possibilities of cooptation and hoodwinking rather than of representation.

The conditions for the rise of corporatism are the growth of multinationals, the increased complexity of integration in a world economy, more sophisticated technology and transportation, and most specifically, the rise of Keynesian economic policies. The formal incorporation of labor into planning and policy development with the state and with other organized private interests is related to the successful electoral performance of left-wing, labor, and social democratic parties, as well as to the increasing success of socialist and "Eurocommunist" parties.

Corporatism is mostly about economic planning and crisis management in a world economy. Its primary successes have been in the development of incomes policies, with much less success in such other policy areas as codetermination, investment planning, and national defense.

Several critical issues and assumptions underlie the concern with corporatism. Perhaps the most important of these is the necessities of bureaucracy in a complex and interdependent world system. This issue is never fully developed in these essays, perhaps because of the political science bias toward what Schmitter calls "interest intermediation." In this respect, I agree with Birgitta Nedelmann and Kurt G. Meier, whose essay holds that Schmitter's analysis of corporatism is on the same level as most of the authors' analyses of pluralism.

The theme of corporatism exists elsewhere in sociology, and I now think it is a very critical issue. Nevertheless, until I read this collection, I had missed much of its excitement. For example, corporatism is about Lipset's emphasis on the themes emerging from a comparison of Marx to de Tocqueville or Weber to Michels; Selznick's formulation of democracy in the midst of bureaucracy; and Milovan Djilas's, James Burnham's, and Tony Cliff's concerns with socialist bureaucracy. Nevertheless, the old models, questions, and solutions will not suffice, as this collection of essays indicates. It is not bureaucracy and representation pure and simple that is at issue, but the planning of an economy, the balancing of inflation and unemployment, the context of liberation movements, Third World penetration into the security of First World Trilateralism, upfront class conflict, and the limits and cartellization of resources. The alternative specters of a communist Europe and the rise of fascist reactions to leftist movements provide a background to the study of corporatism but are not sufficient to motivate it in themselves.

In addition, some of these essays both welcome and fear the possibility of a third way. What if a managed economy did contain and remake the meaning of class conflict? Who would educate the educators? Who would tutor the philosopher kings? Both are interesting and curious ideas to toy with, but alternatively, what if corporatism is another fraudulent reproduction of bourgeois hegemony? Is it really a postindustrial society or just the crisis management of capitalism on the transitional road to socialism? The study of corporatism does not address all of these questions, but it draws into itself some of the primary themes of political sociology and political science: the rise and demise of parties; the relation of parties to

such other forms of interest intermediation as unions and corporations; meaning and means of representation; political outcomes and accountability; culture and history; knowledge versus labor; dominance versus exploitation; interests and organization.

As always, however, some of our most exciting work becomes available just when it seems least relevant. How ironic that corporatism has become a central object of study just when deregulation hits the Trilateral collaborators. The United States, which it is claimed never had corporatism, has even less now, but one might say the same thing of England, Sweden, the Netherlands, and Japan, as well as Spain. Just as scholars have gotten themselves together to study social democracy systematically, it is voted out of office. Just as we begin to think through welfare capitalism, it is rescinded and discredited by its heartiest supporters.

This is a good collection. I thoroughly enjoyed reading it despite its many typographical errors. Undoubtedly the research generated by the ideas presented here will branch out in different directions, with alternative theoretical frameworks. I hope that some sociologists take up the challenge because, as evidenced in this collection, the study of corporatism has been limited by its entrapment within political science where the traditions of that discipline have heavily weighted questions and organization of evidence. That is, in political science representation has the key place—or interest intermediation, as Schmitter calls it—whereas issues of bureaucracy, ideology, community, and quality of life have scarcely been considered.

Studies of the Modern World-System. Edited by Albert Bergesen. New York: Academic Press, 1980. Pp. xiv+281. \$25.00.

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The world-system perspective is new and controversial. It is new because it incorporates a much broader, large-scale, and long-term interpretation of social change. And it is controversial because critics from both the left and the right have accused it of a variety of sins, including oversimplification and terminological ambiguity. The excellent collection of essays in *Studies of the Modern World-System* will provide both supporters and critics with plenty of ammunition to make their cases, but perhaps more important, the quality of the essays and some of the new propositions that they generate may compel the doubters and the uncommitted to take a more careful look at what might be gained through adoption of a world-system perspective. In short, this book will be of genuine interest to the scholarly community.

The world-system perspective itself is treated in a very cursory fashion

in the book, presumably because it is too new to have established a completely acceptable text. Certainly some of the differences in approach and conceptualization that appear in the individual chapters would suggest that the perspective is still in an exploratory stage. Nevertheless, as Albert Bergesen indicates in the preface, there are assumptions that all the authors share (p. xiii). These include the belief that there is a "collective reality" above and beyond individual societies "of one sort or another" with its own "laws of motion" that determine social, political, and economic realities in national societies. This collective reality has structural constants (especially the core-periphery division of labor), cyclical rhythms, and long-term trends. These assumptions extend the traditional international systems perspective, since individual societies are no longer considered the primary determinants of reality and since the focus on long-term trends and constants transcends the more narrow focus of most research on international systems.

The failure to treat the dynamics of the world-system perspective more thoroughly, despite the difficulties of doing so at this stage, seems a mistake. Only the concluding chapter by Bergesen and Ronald Schoenberg, which analyzes "long waves" of colonial expansion and contraction, deals with these wider dynamics. The other chapters look primarily at the impact of the world system on national societies. These chapters are enormously diverse in methodological approach (ranging from analytical history to sophisticated statistical analysis) and in subject matter (ranging from imperialism and colonialism, the world polity, and state dominance to the institutionalization of science in 17th-century Europe, regime stability, trade dependence and fertility in Latin America, and tariff politics and class struggle in the antebellum United States). This diversity provides a variety of illustrations of the new approach, but there is also the attendant danger that the trees will obscure the woods. The danger comes from two directions, since it is not always clear why some of the case studies require the world-system apparatus to reach their conclusions (thus potentially violating a principle of theoretical economy) and since it is not always clear, except in the case of very long-term trends, why international systems analysis could not provide an equally acceptable conceptual framework.

The core-periphery division of labor provides the key explanatory variable in many of the chapters. One strong virtue of this book is that this dichotomy is used with an unusual degree of sophistication. In particular, the narrowly economic focus of dependency "theory" is considerably extended here. As one example, John W. Meyer in a chapter on the "world polity" argues persuasively that the peripheral states have followed the patterns of development of the core states (thus avoiding complete "peripheralization"), a trend that he attributes to the power of modern culture to impose a set of attitudes that legitimates the nation-state and the expansion of its authority (pp. 117-35). Other chapters, especially one by Robert Wuthnow on science, also suggest the importance of noneconomic

factors such as the social conditions that existed in particular countries before their inclusion in the world economy. Still, given the importance of the core-periphery division, one might argue that it obscures many differences within both core and periphery, especially as the world system has become more complex, and that the authors have paid insufficient attention to the economic literature that casts serious doubts on many of its central propositions (particularly the notion of a secular decline in the terms of trade).

As illustrations of innovative ideas, one might cite the argument by George M. Thomas and John W. Meyer in a chapter on regime stability that regime turbulence creates stronger, not weaker, states (pp. 152–54) or the exceptionally interesting finding by Bergesen and Schoenberg that colonialism expands when power in the core is dispersed and contracts when the core is dominated by a hegemonic power (pp. 238–40). But Bergesen and Schoenberg, in arguing that a new wave began around 1973 largely as a result of declining U.S. power, ignore the effects of OPEC on current developments—perhaps because the notion of the periphery undermining the core calls into question the notion of one-way causality. Finally and more critically, in an article on trade dependence and fertility, Michael Hoyt argues that the fertility-reducing effect of development is reduced by trade dependence (pp. 181–84) but ignores the fact that some of the most dependent countries in the Third World have the best records in growth, equity, and family planning. At any rate, the policy prescriptions that he draws from his analysis seem sharply oversimplified.

On the Autonomy of the Democratic State. By Eric A. Nordlinger. Cambridge, Mass.: Harvard University Press, 1981. Pp. viii+239. \$20.00.

Alan Sica

University of Kansas

In *On the Autonomy of the Democratic State*, Eric Nordlinger has written a theoretical and polemical work designed to enrich discourse on the contemporary democratic state among his colleagues in political science. Many of them will probably find it innovative and helpful, but sociologists who know the relevant studies by Perry Anderson, Reinhard Bendix, and Theda Skocpol, in addition to a number of recent Marxist articles treating the state within global capitalism, may well be less impressed. Nordlinger's work, unlike those just mentioned, is virtually free of historical content, does not grow from a recognizable tradition of sociohistorical work on the state, and is tied stylistically to a type of formalism no longer much practiced by sociologists who have escaped the most rarefied structural-functionalism. Nor is it part of the tradition of theorizing about state-society relations initiated by Pareto, Michels, and Mosca, each of whom docu-

mented their hypotheses with generous historical detail. Instead, Nordlinger's book—after carefully *noting* recent contributions on the state by Skocpol, Gianfranco Poggi, Nicos Poulantzas, Claus Offe, James O'Connor, and others (p. 224, n. 5)—carries on in its own stalwart way, with a single message: given the slightest opportunity, political leaders, elected and appointed, will do what they want regardless of opposition from the society "at large" or interest groups within it. This is the "autonomy" in the book's title.

Nordlinger claims that practically all research on the issue of state-society dialectics carried out since the 1950s has worked within the "society-centered perspective." This argument, familiar from both pluralist studies of power and official ideological rhetoric in the democratic nations, holds that governments serve people by serving associations and organizations, and that when they fail to do so, the public replaces them. Consequently public officials behave very cautiously in setting policy and can be counted upon to lack initiative, innovation, or courage in the face of public opposition. Nordlinger believes that this domain assumption, with its implications, has become such an article of faith among social scientists that the opposing view needs to be aired once again. Somewhat surprisingly, given the nature of the book, in the last few pages he ties his revisionism to "the realist study of democratic politics" (p. 209) and mentions the Italian "Machiavellians" and Joseph Schumpeter's dour vision of democratic possibilities as predecessors to his work. This claimed lineage does not seem very plausible, because the research practices evident in the earlier books bear no relation to Nordlinger's. These research practices, I believe, explain why the earlier theorists continue to find audiences among students of the state while so many contemporary works fall quickly into obscurity.

The author's expository strategy is more philosophical than historical or sociological. Nordlinger is adept at drawing from a plausible political situation all permutations, likely and unlikely, historically typical or non-existent, and formalizing them into an orderly arrangement. While Pareto and Mosca composed their theories of the state in response to contemporary events in European politics, informed by considerable historical knowledge, Nordlinger cuts himself free from such limits and allows his imagination to rule. This is not to say his work floats high above the empirical in a philosopher's heaven without contingencies. But its empirical foundation is at points extremely thin, a fact Nordlinger admits but does not find problematic since he hopes his thesis will promote more "state-centered" research. The problem, then, becomes one of rhetoric and scholarly accuracy. What does it mean when an author claims to have "demonstrated" a position? Readers of Bendix's *Kings or People* (Berkeley and Los Angeles: University of California Press, 1978), for instance, expect to find a theoretical claim or substantive generalization amid plentiful evidence. Nordlinger's evidence does not exist yet, he says, because there are

so very few studies that focus on politicians' autonomy and self-direction rather than on their obedience to and fear of public pressure. In fact, the only recent monograph that is mentioned in the book as proof for the principal assertion is Stephen Krasner's *Defending the National Interest* (Princeton, N.J.: Princeton University Press, 1978), a work that highlights a type of governmental behavior in which one would expect considerable autonomy. Krasner's findings reappear to prove various subhypotheses, almost like a *deus ex machina* whenever other data are thin.

So what one confronts in the book are sets of hypotheses, models, options, techniques, and so on, all manicured by the author to appear "patently plausible" (a favorite phrase) but seldom tied to any data, recent or old. (Watergate, the Vietnam debacle, COINTELPRO, and any number of other instances which would seem to buttress Nordlinger's viewpoint perfectly appear nowhere in the book, and one suspects they are too "real" and messy to be included.) Nordlinger lists six propositions, the "plausibility" of which he attempts to prove (p. 7). These are all the logical possibilities, one finds, beginning with "when state and society preferences do not diverge, then. . . ." "Societal preferences" is "a shorthand term for the parallelogram (or resultant) of resource-weighted private preferences" (p. 16), a view which signals Nordlinger's debt to marginalist theory and the voluntarist nature of his thinking. Much more important than the six hypotheses, though, are Nordlinger's three types of state-society relations (pp. 28 ff.): Type III is described first and most easily, since within it state and societal preferences are nondivergent and the state does what it wants. In type II the state's wishes do not at first fit with the polity's, but after skillful manipulation the state defuses the opposition and carries out its policies. With type I, state officials do what they want in spite of opposition, since they do not fear penalties. Nordlinger spends many pages explaining how these relations might obtain in the real world, using to his advantage a number of pluralist, neopluralist, social corporatist, and Marxist studies, none of which, he believes, takes state autonomy seriously enough. But his innovation is a kind of updating of Machiavelli, in the form of 45 "autonomy-enhancing strategies and options," 15 for each type (pp. 92-93, 111-12, 130-32), which officials can use to gain their ends. This is, of course, where the book cries out for empirical support, since without it the lists of "options" become hollow, manipulative tools, logically reasonable but grossly distanced from political praxis. And because of Nordlinger's preference for argument over evidence, he cannot specify how "the state" (frequently reified, apparently knowingly, throughout the text) actually carries out the options, particularly against stiff opposition.

Because of the book's nonstructuralist, voluntarist understanding of the relation between state practices and capitalist needs; because arguments over plausibility overshadow either historical trends or empirical detail; because so much of the book is given to internecine arguments among political scientists stuck in ideological warfare of the mid-1960s; and because

Nordlinger tried valiantly to counter everyone's probable objections to his thesis, thereby serving no one thoroughly or well, I do not think the book is a "major contribution to the growing literature on the state" (as Stephen Krasner describes it on the jacket), but rather a promising appendage to the Machiavellian line. Had the author begun by listing his 45 autonomy-enhancing options and then showed how they are used by politicians and bureaucrats to thwart opposition, he could have guaranteed himself a wide readership. But lots of things are merely plausible; the question is, Why does this event happen and not that one?

Electronic Colonialism: The Future of International Broadcasting and Communication. By Thomas L. McPhail. Beverly Hills, Calif., and London: Sage, 1981. Pp. x+260. \$20.00.

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The "New World Information Order" (NWIO) is the process through which the less developed countries (LDCs) seek to achieve a more equitable balance in the international flow of information, the recognition of "development-journalism" (the self-determination of domestic communication policies) through governmental control, and a more accurate reflection of their affairs in the world's media. This represents a threat to the West's current unlimited freedom to propagate economic and political interests, a practice founded generally on liberal journalistic philosophies. Thomas McPhail's *Electric Colonialism* deals with the Unesco debate between these ideologies during the past decade. The book's detailed insider's account, background material, and conclusions overcome successfully what has been identified as a typical difficulty in the study of communications and development by Elihu Katz and George Wedell in *Broadcasting in the Third World* (Cambridge, Mass.: Harvard University Press, 1977): the need to combine short-range fact gathering with a long-range analytical perspective.

McPhail's command of "Unescoese" as language and as process, and a good investigative reporting style enable him to convey the dynamics at work behind the apparently dull, legalistic Unesco scene.

The power shift to the majority of new members in the early 1970s enabled the LDC bloc to apply pressure toward changing the free (from governmental interference) flow-of-information concept, a usual accompaniment of imported Western communications technology and one which contradicts "development journalism." The weakness and bad timing of Western reactions proved how the United States and its allies had underestimated the impetus of change and reduced their options to the use of

sheer power. This came in the threat of a walk-out by the Western delegation in the 1976 Nairobi General Assembly, in response to an LDC's draft resolution that explicitly called for state control of the media. Temporary compromise was achieved through a dilution of the irreconcilable positions into a more acceptable and vague "free and *balanced* flow" concept (p. 129; my italics) in return for a Western pledge to donate more untied dollars and to remove its opposition to Unesco's sponsorship of further discussion.

Together with the background material, the descriptive chapters help "de-mythologize" Western rhetoric. McPhail demonstrates how the free-flow concept has become unacceptable not only to the LDCs but also to such Western nations as Canada and Australia, where a development-journalism philosophy serves to protect national identity.

The "neutral technology" argument is equally doubtful, as illustrated by the impact of ideological differences on the control of technology. Persuasive examples include the LDCs' demands to reallocate priorities in spectrum management, to abolish the monopoly of Western (and Soviet) wire services on news flow, and to share control of direct broadcast satellites.

These instances illuminate theoretical problems, particularly the Western disregard of social structure as an essential variable in communications research and policy. This not only applies to Western media attitudes and to the export of its models but also is typical of the premises of Western social research. The concern with methodological precision and its consequent microsocial scope and techniques leads to an artificial isolation of phenomena from their contexts, followed by biased conclusions. This is incompatible with the holistic approach required for an unbiased treatment of the individual, organizational, and institutional interactions characteristic of national and international communications. Not dealing with structure and not questioning the system means accepting the status quo, thus passing judgment, a clear contradiction to alleged scientific objectivity. In addition to this display of Unesco's "*emperor's new clothes*," at least two examples in the book demonstrate the improved perspective provided by the concern with structure. In the first, McPhail suggests that the real catalyst for compromise in the Nairobi deadlock was the personal motivation of Unesco's Director-General Amadou M'Bow, who "found his back, and career, against the wall and quickly abandoned what he cherished . . . to pacify the Western nations" (p. 137) in order to "keep Unesco intact—as well as his career path . . ." (p. 110). The second example deals with the MacBride commission, set up at M'Bow's initiative as an outlet for the Nairobi crisis. Its high-caliber membership and pretentious mandate—to "study *all* communication problems in present-day society," as mentioned in *Many Voices—One World*, the final report published by Unesco in 1980 (p. 295; my italics)—conveyed a powerful image and instilled high hopes. McPhail brings it all down to earth in his portrayal of

the commission as being continuously outmaneuvered by Unesco's pro-LDC secretariat (p. 209), the performance of its chairman as little more than a passing media event (p. 126), and its interim report, "the most elaborate and recent statement on the entire New World information order debate" as being denigrated publicly by M'Bow himself for "having nothing to do with the 'Nairobi' media declaration" (p. 125). True, the importance of personal motives in ideological conflict and the establishment of study commissions as expedient devices are well-known, but McPhail shows the difficulties in grasping the dynamics of such informal processes. He also indicates ways to overcome them, through an awareness of structural context and a meticulous case-study method. It is doubtful whether the formal nature and the close focus of microsocial methods would enable one to penetrate such blanketed layers of information.

Although the book has indisputable merits, it leaves several expectations unfulfilled. Although development journalism is described empirically, one learns less about its philosophical bases than about those of the Western positions. The "Freedom of the Press" chapter and the detailed evaluation of Western media and development research traditions are not counter-weighted by the much narrower "Structuralism" and "New Departures" sections (pp. 77-78, 83-85). These passages present the new approaches as little more than educated critiques of Western theories, with only incidental references to their constructive aspects. Equally surprising is the absence of criticism of these radical approaches, or at least of their difficulties in analyzing specific countries and cultures. Such problems may be due to their exaggerated emphasis on global systems, aggregate indexes, and deductive procedures. (See Alejandro Portes, "On the Sociology of National Development: Theories and Issues," *AJS* 82, no. 1 [1976]: 55-85.)

One would also expect an evaluation of the NWIO and its future. Although the remarks presented in the concluding chapter form quite a relevant agenda, they do not deal with the crucial issues of whether the NWIO is indeed a full alternative model or simply an elaboration of the radical approach that gives disproportionate attention to technology and control; and of whether the NWIO will be able to create its own "media culture" with contents, formats, style, and professional norms to replace those typical of imported Western models.

These shortcomings and some irritating misspellings of non-American names, such as those of Latin American social scientist Luis Ramiro Beltran on pages 70 and 73 and Finnish ex-President Kekkonen on page 95, do not detract from the book's importance for a better Western understanding of the "world out there," of its right to choose its own way of life, and of the need to devise equitable means for international coexistence.

Television and Social Behavior: Beyond Violence and Children. Edited by Stephen B. Withey and Ronald P. Abeles. Hillsdale, N.J.: Erlbaum, 1980. Pp. x+356. \$24.95.

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In December 1972, shortly after the publication of the U.S. Surgeon General's report on television and social behavior, the Social Science Research Council (SSRC), responding to encouragement from the National Institute of Mental Health for a follow-up report on television, formed the Committee on Television and Social Behavior. The committee was established to assess the current state of television research and to make recommendations for new research. Out of its three-year program of activities grew *Television and Social Behavior: Beyond Violence and Children*, a volume of 13 essays written by the committee's members and consultants. The book is an unusual committee document which exceeds the standards of most conference reports and one in which individual efforts appear to have profited from group discussion and review.

A consensus was not reached on the direction in which the diverse field of mass communications should be heading, however, and there is little or no linkage between one chapter and the next. The editors, Stephen Withey and Ronald Abeles, ask to be excused for the lack of synthesis, stating that the field "lacks conceptual coherence" and that it is "principally a melange of diverse research interests" (p. 293). The book reflects this state of affairs, presenting the reader with a series of lofty ideas about television in which the whole is directly equal to the sum of the parts. The parts, by and large, are excellent.

An ambitious and thoughtful analysis by Jack McLeod and Byron Reeves on the methodological issues facing the field is followed by four chapters which scrutinize various aspects of the television industry. Herbert Gans examines the relationships among the television product, its audience, its producers, and its researchers and speculates on the effects and policy ramifications of this "low involvement" medium. He suggests that television researchers assess the potentials for bias inherent in their particular research perspective. No one, for example, sets out to explore the "bad effects of high culture" (p. 78), notes Gans, and yet only recently have people thought to look at the potential "prosocial" effects of mass media.

Paul Hirsch's analysis of the television industry, along with its products, as an economic institution, sheds light on the pure business rationale behind programming decisions and advertising. Within each day and time slot, the size of the aggregate television audience remains the same. Hirsch deduces that a large proportion of viewers watch television in general rather than programs per se and that, contrary to McLuhan, who saw the

technology of the medium as the message, it is instead the "underlying myths, shared values, and rituals" across the programs themselves that become the message of television (p. 97). Thus, viewers seek familiarity and predictability in television and try to avoid those rare instances that violate their expectations. In the next chapter, the history of the television industry's policy on violence is carefully traced and the overall impact of the Surgeon General's report assessed by Leo Bogart. He expresses concern for the industry's tendency to hammer out programs according to specifications that often call, unnecessarily, for violence. Nonetheless, he concludes positively with the contention that "social research can influence social policy" (p. 131). In the last of these four chapters, Hilde Himmelweit (the only foreign member of the SSRC Committee on Television) discusses the interdependence of the industry, the audience, and other social institutions. Highly aware of the differences between British and American television, she encourages researchers to recognize, if not abandon, their ethnocentrism when thinking about research and findings. Television, she argues, must be viewed as a creature of its culture. She also invites more research on the effects of new telecommunications technologies, a wise suggestion which, unfortunately, was not followed in this volume.

The committee's interest in the television portrayals of minorities is covered in three papers. Gordon Berry documents the history of racism and black archetypes in American media. He charts three major periods in the portrayal of blacks on television: (1) the stereotypic age—1948–65; (2) the new awareness—1965–72; and (3) stabilization: the settled phase—1972 to the present. Sherryl Brown Graves presents a thorough and workmanlike review of research on the psychological effects of black portrayals and offers eight areas for further research.

The most intriguing contribution of this triad is that of a black psychiatrist, Chester Pierce, who carefully describes his personal reactions to nuances in television's treatment of blacks which, he argues, remains racist and denigrating. My initial response to this probe of the subtleties of television content was that the author was overly sensitive. But on reading further, my appraisal shifted to an appreciation of his observations. In short, my consciousness was raised. I had thought that television had made significant strides in eliminating racist stereotypes, but Pierce showed me that "social trace contaminants" remain in contemporary American television.

While these three papers are excellent, I recall that the SSRC committee had expressed "strong interest" in examining television's influence on "special audiences" (p. viii), citing the elderly and minorities specifically. One wonders, then, why the three chapters on special audiences focus solely on blacks and why no papers on women or the aged were commissioned. The television research literature on these two groups is expanding, and coverage in this book would have been appropriate.

Irving Janis's contribution focuses on how people may use television in

the process of personal decision making. The tack here is unusual in that the chapter goes just where the book's title promises: beyond the obvious areas of pure modeling theory, violence, and children. It speculates instead on the intersection of television content and the multitude of decisions that face people daily. Finally, Aimée Dorr's superb chapter, "When I Was a Child, I Thought as a Child," presents more food for thought than any of the hundreds of published articles on children and television. Dorr dovetails what we know about the child's cognitive construction of the world with what he or she sees on television. She breaks new ground speculating on what meaning the child derives from the grammar and syntax of television. For example, does the child recognize an advertisement as distinct from the program in which it is embedded? How does the child comprehend such effects as "flashback" or "slow motion"? Does a character seem more "real" to the child when the character is "spun off" or appears on another program but is still "in character"?

Dorr places herself successfully in the mind of the child and effectively conveys to the reader how a young child might perceive the world of television. Like many of the contributors to this volume, Dorr raises an array of researchable questions about television that are important for both child and adult viewers. A great many of these questions are new and encourage us to think about television in new ways. A fine companion work to this volume, incidentally, and one which emerged from the same committee and publisher, is *The Entertainment Functions of Television*, edited by Percy Tannenbaum.

The Politics of Population in Brazil. By Peter McDonough and Amaury DeSouza. Austin: University of Texas Press, 1981. Pp. xii+178. \$19.95.

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The Politics of Population in Brazil deals with attitudes and values about population planning among the Brazilian elite and the public. It is of special importance to social scientists because, unlike the majority of studies dealing with population issues in Third World countries, it is not a manual for planners but a true sociological study. Except in parts of its weak conclusion, this book does not emphasize methods of implementing family planning programs, but inquires why Brazilians approach family planning as they do. This study is based on interviews conducted in 1972 and 1973 with 269 members of the elite groups and 1,147 members of the general population in the southeast region of Brazil. The findings are reported in five chapters. The heart of the study lies in the three central chapters

which present the opinions of the public and the elite on family planning and the elite's perceptions of the public's opinion.

The Brazilian position on family planning has been one of *laissez faire*. There are no official family planning programs. Even recently proposed changes do not differ much from previous policy in this area. Great emphasis is placed on maternal and child health care, and some provision is made for furnishing information about fertility-control methods. However, these governmental actions fall far short of taking a strong position on family planning.

Why has the Brazilian government done so little in terms of an official family planning policy? According to Peter McDonough and Amaury DeSouza, the Brazilian elite has taken this position because it does not believe that there is an important population problem in Brazil. Furthermore, the elite does not consider family planning a major component in the solution of Brazil's socioeconomic problems. Nor, in the estimation of the elite, does the public put priority on population policy in its demands. How does the public feel about the issue? As the authors point out, it is a "bread-and-butter issue" (p. 116) and is considered very important, especially among women. In their concluding chapter the authors, moving in the direction of most studies about developing countries, advocate a family planning program. By discussing this in their concluding chapter, the authors tend to play down the finding in the other chapters that, although the Brazilian public considers family planning important, the issue retains a strong moral dimension and is regarded as a matter of private choice in which drastic government intervention is not welcome. The public sees family planning as a service that can be used or not according to individual option. Furthermore, the authors find that the public places greater priority on land reform (which is viewed as the means by which a cheap and abundant supply of food will be made available) than on family planning programs. The public mentions family planning as an important socioeconomic issue, together with income redistribution, only after it mentions land reform. Moreover, the authors' insistence on the necessity of family planning does not seem completely justified in light of the fact that according to the 1980 Brazilian census the rate of growth of the Brazilian population decreased from 2.9% in 1970 to 2.5% in 1980.

Finally, a word about the sample used in this study. In an authoritarian regime like the Brazilian one, where the military have the final word on policy matters, the absence of the military in the elite group selected for this study raises some questions about the representativeness of the elite sample. Difficult as it must have been to reach and interview these people during the early seventies, I still regret the absence of their views in this study. I hope, like the authors, that other elite members who were interviewed had attitudes and perceptions similar to those of the military, so that the elite sample was not biased.

The Changing Demography of Spanish Americans. By A. J. Jaffe, Ruth M. Cullen, and Thomas D. Boswell. New York: Academic Press, 1980. Pp. xii+426. \$31.00.

Marta Tienda

University of Wisconsin—Madison

A. J. Jaffe, Ruth M. Cullen, and Thomas D. Boswell have prepared a statistical reference book which will be widely used by the growing number of individuals interested in how Hispanic groups are incorporated into the social structure of the United States. Had *The Changing Demography of Spanish Americans* been published by the U.S. Government Printing Office under the auspices of the Census Bureau, I would not complain about the choppy prose, the overly cautious and atheoretical discussion of group characteristics, the tedious detail in presenting tabular information, and the limited explanation of findings. But these sins are unforgivable in a publication by a major academic press. There is no excuse for the glaring absence of a coherent theoretical framework and a sociologically informed interpretation of empirical data. This study ranks among the lowest one-third of studies in social demography.

A book published after the 1980 census but based largely on 1970 census data might seem a bit dated, but the authors try to deal with this problem by using more recent CPS data in their last chapter devoted to a discussion of the characteristics of the Hispanic population. Unfortunately, no further insight into the forces of change is provided, and the book ends abruptly without a closing statement or even a cursory discussion of the significance of the most recent findings. Chapter 3, which provides a concise overview of the contents of the detailed empirical chapters, is the only one that compares the five Hispanic origin groups in a somewhat systematic fashion and could have served as a sort of conclusion.

The three themes that organize the book are (1) heterogeneity within and between Hispanic groups; (2) convergence between Hispanics and non-Spanish whites; and (3) the role of education as a motor of social and demographic change. Of the book's 10 chapters, half are devoted to a detailed description of five major groups: Hispanos (chap. 5); Mexican Americans (chap. 6); Puerto Ricans (chap. 7); Cubans (chap. 8); and Central/South Americans (chap. 9). I was pleased to see Hispanos treated as a group apart from Mexican Americans, but quite annoyed that the five chapters about specific nationalities follow a rigid blueprint determined by the seemingly endless compendium of tables. Instead of adding coherence and balance, this approach exposes the weaknesses in the authors' knowledge of the different groups and the inadequacy of their data. Also this format exacerbates the rote manner in which the extensive factual data are presented, leading to much unnecessary repetition.

Two brief chapters, a seven-page introduction and a 10-page discussion

of "theoretical" issues, serve to introduce the morass of statistical information contained in chapters 3 through 10. These preliminary chapters are poor in socioecological content and writing style. Although the exposition in them seems geared to beginning students of ethnic stratification, the tedious statistical detail that follows, while not complicated or particularly sophisticated, is not likely to be well received by either such students or by individuals who know something about ethnic stratification. The authors clearly contradict themselves by denying their affiliation with an assimilationist framework (p. 17) while hiding behind new terms—"ragout" and "natural life cycle"—to represent their belief in the inevitability of convergence. Readers will have to decide for themselves what is "natural" about the process of convergence and what is theoretical about part 1 (see preface, p. xi) because Jaffe and his collaborators are sketchy on these points.

Space constraints compel me to resist the temptation to engage in detailed nit-picking about abundant technical and stylistic problems and instead outline the book's most serious shortcomings. First, it desperately needs a theoretical framework with which to organize and interpret the key aspects of social and demographic diversity observed among the groups, and especially the differences in patterns of convergence with non-Spanish whites. Education is identified as a motor of change, yet we are not told that it operates differently for the five groups or why this should be the case. There are many other loose ends that need to be addressed. For example, what does a Spanish surname represent, substantively and theoretically, that can help us understand its relationship to, say, fertility? In other words, it is not clear why "both more schooling and a non-Spanish surname result in lower fertility. Each contributes a 20% decrease" (p. 96). Such examples of mindless empiricism abound in this book and diminish its attractiveness by making it neither a solid statistical profile nor a theoretically informed sociological piece.

Second, the book needs some multivariate analyses to broaden its appeal within the academic community. This means extending the analyses beyond two or three variable standardization exercises and attempting to delineate and test causal models. These should include "structural" variables as well as individual attributes. Although there are occasional references to "structural" determinants of change or "structural" correlates of variation throughout, the authors do not clarify what they understand by structural factors or how these may differ (and they do) among the Hispanic nationalities. Existing studies show this to be a serious omission.

Finally, the authors should have attempted to interpret the methodically elaborated differences among the groups. This would have been facilitated by organizing the book around topics—fertility, labor force participation, education, earnings, intermarriage, geographic distribution—rather than around national origin groups. By emphasizing similarities and differences among the groups, this format would have forced the authors to explain their findings rather than engage in rote description with limited or no

explanation. I do not intend to belittle the value of descriptive studies, but given the plethora of studies about Hispanics that have appeared in the past several years, the last thing needed is yet another descriptive, poorly written statistical compendium. To be sure, research about Hispanics is less extensive than that about blacks, but the state of knowledge is more advanced than is indicated by Jaffe, Cullen, and Boswell.

In sum, I deeply regret that the demand for a book of this title (although not of this kind) is so great, because *The Changing Demography of Spanish Americans* is a poor showing in many more ways than I have space to recount. It is certainly not worth the asking price of \$31.00.

Inequality in an Age of Decline. By Paul Blumberg. New York: Oxford University Press, 1980. Pp. xv+290. \$15.95 (cloth); \$5.95 (paper).

Caroline Hodges Persell
New York University

Paul Blumberg has written a significant and very readable book about the state of the American economy and its effects on the American class structure. He begins *Inequality in an Age of Decline* by presenting two competing views of the nature of the U.S. class structure—the convergence and the stability views. Class convergence theory holds that for some time egalitarian trends were pushing the American class structure in the direction of reduced inequality. Blumberg argues that convergence theory dominated American sociology after World War II. The belief was that inequality was declining because of major occupational and industrial changes, including the movement away from the production of goods into services, the expansion of white-collar employment, and the shift in the basis of control away from property and toward expertise. Convergence theorists suggest further that these changes eroded distinctive class subcultures, reduced class consciousness among American workers, and lessened political inequality. Social analysts who reject the class convergence view agree that the general standard of living has risen for all social classes but see little or no reduction in the amount of inequality between classes, as reflected in the distribution of family income and housing, for example.

The conception of social class in this book is not as explicit as it might be. Blumberg seems to view social class primarily in terms of consumption and expenditures rather than in terms of ownership of the means of production or social location in the productive process. His heavy reliance on income and consumer behavior may result from his use of existing data from government and other sources. However, if that is the case he should have indicated his preference for better data not available to him.

Both stability and convergence theorists agree that absolute incomes and living standards increased continuously after World War II. Blumberg

argues that the period of unbridled economic expansion ended abruptly in the early 1970s and that "the living standards of the American people, considered as a whole, have stagnated for more than a decade" (p. 66). Several of his figures about decline in the 1970s have been challenged by such economists as Lester Thurow (*The Zero-Sum Society* [New York: Penguin, 1980]). Thurow and Blumberg agree on the dismal prognosis for the 1980s and 1990s, although for a reason that Blumberg does not discuss, the fact that wives of men with high earnings are entering the labor force in growing numbers.

Blumberg spends a good part of his book discussing indicators of the decline of the American economy, including the plunging value of the U.S. dollar, growing trade deficits, the decline of the steel industry, the increasing importation of manufactured goods, and the disintegration of the American infrastructure of highways, sewers, water, and electrical power systems. He then makes an effort to explain why this decline has occurred. Some of the reasons he suggests involve the heavy U.S. military expenditures that fail to provide economic use value, encourage cost overruns, discourage efficiency, and divert funds and research talent away from research and development in the consumer sector. The policies of U.S. corporations have contributed to the economic decline, Blumberg feels, by selling "front end" technologies to foreign competitors who use them to manufacture products that immediately compete in the American market and by building manufacturing plants abroad because of the cheaper labor. The result of these processes has been "nothing less than a global redistribution of industry, as manufacturing moves out of high-wage developed nations such as the United States and some European countries, and into lower-wage developing nations" (p. 161).

It is difficult to tell from Blumberg's exposition what he sees as indicators of economic decline and what he sees as causes. Is the balance-of-payments deficit, for instance, an indicator or a cause of decline? Also, his conclusions about the causes of decline, for example, military spending, are plausibly argued but not demonstrated conclusively. Rival explanations, such as the decline in cheap energy or rising labor costs, are not considered seriously, with the result that they are not convincingly eliminated. Despite the general cast of his title, Blumberg does not address the question of international inequality. The book is written from a clearly American perspective.

After his stark analysis of the transformation and decline of the American economy, Blumberg's conclusion seems somewhat surprising and abrupt. He proposes a cultural and ideological response to economic decline and urges Americans to strengthen democratic and other values, without addressing possible changes in the economic base on which such political values may depend. He also urges Americans to accept a more cyclical view of history, to realize that the wheel of fortune has turned yet again, and to accept their decline with grace and civility.

On balance, while economists may not accept all of Blumberg's statistics

and there may be limits to the logic of his argument, he has given a vivid description of some major economic trends of our age and has presented us with a basis for formulating our own agenda for action.

Between Public and Private: Lost Boundaries of the Self. By Joseph Bensman and Robert Lilienfeld. New York: Free Press, 1979. Pp. xii+196. \$13.95 (cloth); \$7.95 (paper).

Kim Lane Scheppele
Bucknell University

Privacy has been inaccessible to social theory. Part of this is, of course, due to the fact that our standard ways of finding out about the world (interviews, observations, records) do not tell us much about privacy. And part of this is also due to the conceptual unpreparedness of social theory. We do not know how to approach the private, the secret, the unshared or minimally shared elements of human experience.

In *Between Public and Private*, Joseph Bensman and Robert Lilienfeld's concern is to describe the emergence of and tension between public and private roles—across time, across cultures, and across subgroups within cultures. Public roles, they assert, allow us to project only very limited aspects of our selves; a fuller, more honest presentation of these selves occurs only in such private roles as are found in peer groups, families, friendships, and more intimate relationships. These private roles emerge in response to the needs that most of us feel to express our individuality, to flout the public role-specific norms constraining us, and to seek some solidarity with sympathetic others who will tolerate the gap between our public performance and our private feelings. There is, therefore, a constant and unavoidable tension between our public and our private roles.

One difficulty with the Bensman and Lilienfeld book is that the theory-to-data ratio is far too high. It becomes difficult, in sociology as in arithmetic, to figure out exactly what one has when the denominator approaches zero. Bensman and Lilienfeld assert, as though it were obvious, that "primitive society was dominated by a high incidence of public roles" (p. 36) and that "those individuals who achieve or are ascribed to 'higher' class or occupational roles will find a greater percentage of their total behavior defined by public roles" (p. 19). Perhaps these things are true, but little hard evidence is given for either. And nearly 200 pages of such generalities can be quite numbing. One wants to keep asking, "Which specific societies?" "Under which specific conditions?" and "How do you know all this?" But detailed case studies, illuminating references, or interesting anecdotes that clarify abstract points are almost entirely absent. All one has, to quote mathematician Lewis Carroll, is the "grin without the cat."

This may not, however, be reason to condemn the book entirely. After

all, there are some traditions in sociology where the precision of one's facts is less important than the elegance of one's conclusions. And, as has already been noted, the area of privacy does present quite formidable data-collection problems. What theories do Bensman and Lilienfeld offer us?

One contribution that could be made by a book about privacy would be to figure out, for once and for all, what we should mean by "privacy." Philosophers and legal scholars puzzled over this question for some time (although Bensman and Lilienfeld do not cite either group). Privacy is a particularly tangled concept, but Bensman and Lilienfeld have done little to unknot it. We are told that privacy "refers to the individual in relation to himself" (p. 28); that there is "psychological privacy" (p. 30), "ecological privacy" (p. 31), and "physiological privacy" (p. 54); that "the deepest sense of privacy is the unconscious" (p. 126); and that finally "it is possible . . . to conceive of the private . . . as a biological, physiological and psychological category, upon which social definitions are overlaid" (p. 173). Privacy in this book suffers from an embarrassment of meanings.

A theory with an unclear central concept begins at a disadvantage, but let us look for the sources of Bensman and Lilienfeld's ideas. In their introduction, they claim they will put Goffman's microbehavior in an institutional setting, introduce Merton to psychology and biology, and add a cultural, social, and economic thrust to Freud. What they do, in fact, is assume that somewhere in the course of history individuals were able to develop selves which can exist in rugged opposition to socially prescribed role behavior. These selves express themselves more completely in private than in public roles. "Role" here is the key term, for Bensman and Lilienfeld are locked into the straitjacket of role theory. Consequently, they come up with the unremarkable conclusion that private roles also have their own normative systems that outline approved and disapproved behavior. The authors make some exploratory attempts to outline what expectations exist in intimate and private role relations, but here they are hampered by a lack of data. The life they outline is one which is quite American (certainly not all societies are as self-disclosing as ours), very upper middle class (where middle-aged friendships exist primarily to share successes, provide comfort against failures, and provide opportunities to flout official norms in private), and most certainly male.

The male bias is particularly disappointing in a work of this kind because women have had quite different experiences from men in reconciling the public and the private. Much of contemporary feminist theory reveals the segmental public/private world with which men identify and contrast it with a more holistic, integrated world which women have constructed. To put it in more sociologically familiar terms, the transition from *Gemeinschaft* to *Gesellschaft* was experienced primarily by men; women's lives have been more typical of lives in *Gemeinschaft*. Integration of the public and the private is much more possible in a closely knit community. Bensman and Lilienfeld, by using role theory which divides the self into

discrete and separable entities, deny in a real sense the possibility of this integration. And they conclude that an irreconcilable and pervasive tension exists between the public and the private. Their conclusion is, to a large extent, predetermined by their choice of theory.

What, then, do we have that is new after reading *Beyond Public and Private*? For one thing, the book demonstrates that making privacy more accessible to social theory may prove to be a more difficult enterprise than we might have thought initially. For another, we can see that, however difficult facts about privacy may be to come by, we will need something more solid on which to base our theorizing. And finally, we can see again that a theory that tries to explain everything winds up explaining nothing.

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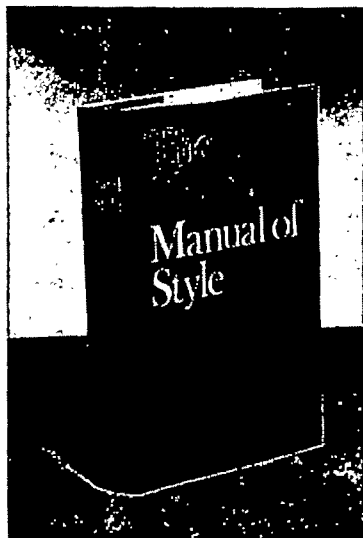
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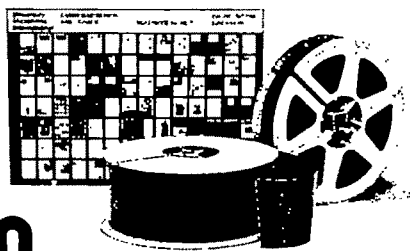
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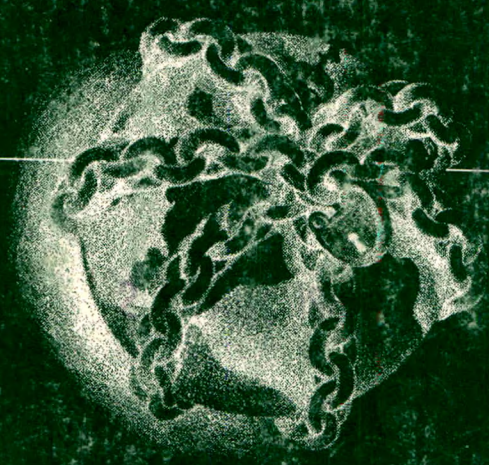
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